The Agricultural Impasse

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THE EFFORTS OF ALL PEOPLES to achieve a place in the sun have turned around the land and what it produces. Among the underlying causes of war, no factor is of importance nearly equal to that of the relation of a population to the productiveness of its lands. This relation involves a complex equation between the utilization of ores, molecules, and atoms and the handling of plants and animals which supply the energy for living. No course can more surely invite disaster than one which fosters headlong improvement in peace and war of industries which consume the products of the land while it all but ignores our relatively poor success in handling the land and its living cover.

The source of man's energy is the green plant. Maintenance of his energy supply or its increase depends upon apt handling of his plants. If he fears the cutting off or liquidation of this supply he will fight for its preservation. The adequacy of the supply and its distribution is, in the last analysis, the key problem. Today we have an alternative to the age-old unbalance of the fat years and the lean, the haves and the have-nots, and the birth of countless millions of human beings to the misery of inadequate food—we can replace "farming" with agriculture, the science of handling the land and its plants and animals.

Great progress in such a revolution has already been made. American agriculture, quite as much as American industry, helped to turn the recent tide of war toward the victory. Many of the peoples of ravaged Europe and Asia owe both their freedom and their postwar survival to the American farmer. But we are moving too slowly. The pressure of war and postwar conditions has so increased the drain of soil fertility and the effects of erosion and inefficient culture that these and related factors are reaching an emergency status earlier than they would have, had a normal peacetime pace prevailed. In much of our supporting agricultural research, too, we have reached an impasse; and in this there is great danger, for the laws of energy will not indefinitely permit increase of demand, without parallel increase in the available supply.

From any long range view, further lifting of the world standard of living can only follow increases in agricultural efficiency. In fact, because of increasing population pressures, even maintenance of the present standard demands a very considerable increase in this efficiency. The Food and Agriculture Organization of the United Nations estimates increases (over previous production) of 21 percent for cereals, 100 percent for milk, and 163 percent for fruits and vegetables as minimum requirements for feeding the expanding world population by 1960. The land available for growing these increased crops has shrunk from deterioration and misuse. Moreover, the hope, often expressed, that such increases can be produced in tropical countries overlooks the severe limitations of these regions. In most tropical regions there is, so far as agriculture is concerned, relatively little usable land. There are no great prairies, and because of the topography only limited areas can be turned to the production of energy crops. Continued high temperatures and excessive rainfall restrict both the types of plants that will grow and man's efforts to grow them. Such conditions also often increase the magnitude of disease and insect problems to nearly uncontrollable proportions. The production of the tropics is destined to be of growing importance with respect to many crops, but the resources of the tropics are simply not such as to make the regions capable of assuming a major position in the production of the energy crops. We must look instead for means of greatly increasing the production of Temperate Zone farms.

The Basic Problem

In emphasizing the need for conservation, it is common to picture the land as a potential resource, a sort of agricultural safe deposit box from which man may draw fertility for his crops for an indefinite period. If he withdraws this fertility too rapidly, however, or allows the mineral elements to be washed away by erosion, he must move on to new land or, when new lands are no longer available, face ruin. All this is true, but it is a concept too limiting to serve as the basis of a scientific agriculture. It is necessary, instead, to think of a farm as a factory and the farmer's managing efforts as attempts to turn raw materials into fine goods, soil elements into plant products. The goal is the most rapid practical turnover of fertility, the highest production consistent with continual availability of raw materials. Many of our soils lack certain essential elements, but most of them contain, in

practically inexhaustible amounts, all the required elements except nitrogen, phosphorus, and potassium. Agriculture becomes then in part a matter of supplying phosphorus and potassium as crude chemicals in the right amounts, fixing nitrogen chemically or by the activity of microorganisms, and providing in one way or another such additional elements as may be required to enable the protoplasm of the living plants to manufacture, in mass production, fine goods.

This is the salient fact. In practice the operation is exceedingly complex, and we are in discouraging ignorance of most of its components. The soil in which many key processes go on is a variable and little understood medium, the steps in the functional processes by which plants convert materials from soil and air and the energy of light into fine goods are nearly complete mysteries, and the intricate balances of soil, plants, and climate have hardly been approached. We know only enough to realize how much needs to be learned and to recognize that if we had more knowledge we could make much better adaptions, maintain much better balances, and control the hazards more effectively.

Effective long range use of the soil with efficient cultural practices and adequate control of diseases and insect pests are pressing agricultural problems, but there are others. True to the American pattern, we have made greatest advances where mechanization fits into agriculture. In many crops all operations from sowing to harvesting are now carried out by machines. One effect has been to call for greater efforts to improve plants; for the more highly developed agricultural practices become, the more urgent is the need to produce more efficient plants—plants which can produce more fine goods per unit of raw material, or which, better fitting the pattern of mechanized farming, can produce more cheaply.

Advances in soil practices, control of plant diseases and pests, mechanization of farming operations, and plant improvement already have been great, but not so great as not to be dangerously dwarfed by industrial advances and the factors of population increase of the past several decades. Thus, an unhealthy situation arises. The more the efficiency of industry is increased and the higher wages it can pay, the more difficult it becomes for the less efficient agriculture to find labor and capital. The more efficient industry becomes, the more it expands and the greater becomes the need for new or better agricultural products; and even apart from industrial development, the more population increases the more people agriculture has to feed and clothe.

In any analysis of American agriculture's besetting difficulties a single fact stands out above all—the need for a coordinated and effective program of research.

Agriculture is still, by and large, in the hands of individual farmers with access to relatively little capital as compared with large industrial corporations. Even in good times, individuals are rarely capable of supporting research and experimental development. Then too, the farmer's immediate problems are generally tied to a particular piece of soil in a specific climate, and to the caprices of the weather. It is difficult for him to think in the broader terms which characterize modern basic industrial research.

What is termed industrial research really represents two usually more or less distinct elements. The general fact finding is perhaps accomplished most often by investigators outside the industrial laboratories, by the workers in "pure" sciences, most often in university laboratories. Industry carefully currycombs the findings of such investigators and then tests any interesting facts revealed. Agriculture has no adequate provision for either part of this operation.

The fundamental unit in agricultural research is a living plant or animal and its products. This fact gives a distinguishing breadth to agricultural research needs. Plants and animals are all composed of a common stuff and they all grow according to similar patterns. As a result, research on corn may prove to be just as important for the cotton farmer as for the corn farmer. This means that one has to think in specific terms of cotton or corn and at the same time in general terms of living organisms. The biologist investigating a specific problem in corn soon finds himself knocking at the closed door of what life and growth and sensitivity are. His field is far more mysterious, and, one suspects, far more difficult than those of his colleagues working with inanimate atoms.

The peculiar status of agricultural research needs has been recognized for some time. It was in response to this recognition that the plant and animal research branches of the United States Department of Agriculture and of the various state experiment stations were organized. We have in the correlated branches of these agencies a structure which is theoretically capable of dealing with both short range research, concerned with solution of the farmer's immediate problems, and long range research on fundamental problems. In actual practice, it does a good job at the former and shies away from the latter. The American taxpayer is putting millions of dollars annually into agricultural research. He is getting progressively smaller returns, and too few of these are the kind that will pay off in the long run. Why?

Federal and State Plant Research

So far as the national picture is concerned, crop plant research is centered in the Bureau of Plant Industry, Soils and Agricultural Engineering of the

United States Department of Agriculture. This agency is composed of several hundred professional plant scientists, operating on a budget which, counting both direct and indirect appropriations, approximates ten million dollars a year. Its staff probably contains between one-quarter and one-third of the country's plant scientists, the remainder being distributed fairly equally among other federal agencies, the state experiment stations, and the colleges and universities.

Agricultural research is almost necessarily a ward of the state, and this federal bureau should be the dominant agency in plant research—stimulating fundamental research by its own research productiveness, on one hand, and guiding the application of newly discovered principles on the other. Actually, its accomplishments in the first field are considerably less than they might be, though at one time the Bureau played a much more important role than it now does.

Some of its deficiencies are fairly easy to indicate. Its staff is made up of a very considerable number of topflight scientists, but it also has more than a few mediocre persons who value the security of federal civil service retirement more highly than real scientific accomplishment. Unfortunately, the system is such that the potentialities of many of the best men are not realized. Too often the civil service system and good productive research don't mix. This is a serious business, for it has driven many of the best of the wartime federal scientists out of the government service into industrial or university work, and usually away from pressing problems concerning the national welfare. Many factors are involved. One is bureaucratic insistence upon establishing organizational patterns without appreciation of the need for flexibility in research. Another is the too frequent reservation of higher grades for administrative employees. The effect of this is to make administrators out of the more capable men, and conversely to keep actual research activities in the hands of the less capable, or less advanced. Productive research is a coupling of ideas with experimental techniques. Only in rare instances can each component come from individuals at different levels of responsibility, separated perhaps by thousands of miles. Seniority, so necessary to maintenance of a good organization, often precludes obtaining the best qualified investigators for particular problems. The civil service efficiency rating system, though it is handled in general with attempts at the utmost honesty, often complicates the picture, for the employee's advancement chances turn directly upon it. It is probably possible to make estimates of the comparative efficiency of twenty-five typists working in the same office. In fields of scientific endeavor, however, such comparative ratings are literally impossible. You simply cannot measure a man's scientific productiveness in terms of his industry, his ability to get along with his superiors, the degree of neatness with which he conducts his work, and so on. What you have to evaluate is his brain power. What the civil service system evaluates is his capacity to fit comfortably into an organized pattern. The organization is the thing. If a man fits it—well, let's not worry too much about the fact that he likely is not the most brilliant man in his field. Some signs are appearing which seem to indicate fettered steps on the part of the Civil Service Commission to attempt some modifications of its system to deal with the needs of research agencies. This could be hailed as progress were it not that the organizational difficulties of the agricultural research bureaus would likely be self-perpetuating even without civil service.

But the personnel difficulties are not the only important ones. The present plant scientists of the federal system could turn out far more productive research if they were not hampered by the nature of the system itself. Let's examine the system.

A group of working-level scientists conceive a research project which they feel, on the basis of their specialized training and experience, may reasonably be expected to answer some questions which are bothering them in their attempts to improve a certain crop plant. They draw up an action plan, with all the necessary attention to organizational detail. The plan is reviewed by their division head and the chief of bureau, with his assistants acting as advisors. If approved by these men, it is sent on for approval by the Agricultural Research Administration. This is a superstructure agency whose functions include overseeing the work of all the Department of Agriculture research units and their cooperative work with state agricultural groups.

The concept behind the organization of the Agricultural Research Administration was that it should coordinate the efforts of different groups, eliminating unnecessary duplication, and in general tie together the work of federal and state agricultural scientists, and guide their research along profitable lines. It was a noble concept. With vision and a degree of freedom, the Agricultural Research Administration could lead all the country's agricultural research, stimulate important allied researches, and very effectively raise agricultural efficiency, not only in this country but over the world. It could be one of the greatest factors in our world leadership, for little we could do would be of greater benefit to mankind. Actually, the task of the Agricultural Research Administration is, at least in part, one of dignified pussyfooting. To it falls the responsibility for sorting out of the federal agricultural research proposals those which Congress

will approve and shunting the others to the side lines. It is the liaison agency attempting to maintain long term productive research against the backdrop of Congressional political interests. It must see that only projects likely to have enough Congressional support or public interest are presented for consideration, for the support for the research comes, with a few minor exceptions, from the public treasury. The funds are authorized by Congress on an annual appropriation basis. These appropriations, woefully inadequate if judged against the magnitude of the problem, are relatively small items in the great Department of Agriculture Appropriations Act, which provides for the many important and unimportant services of the department.

Here is a major difficulty, for the basic problems of plant improvement are hardly likely to be intriguing to either Congress or the general public. The Agricultural Research Administration attempts to be certain that the scientists never use language which would sound as though they were talking down to the legislators. Hence its scientists, to start a research program, must sloganize it, must optimistically forecast great results from it, and report its progress in journalese. In short, prosaic scientific research, whose interpretation requires the precision of its own scientific terminology, must be dramatized—and in words of one syllable. The Agricultural Research Administration cannot permit an attack on problems that Congress might feel are too remote. Then there are issues to be attended to like the demands of the state agencies for research assistance. With such responsibilities the Agricultural Research Administration has little time for anything like deep-delving consideration of basic agricultural research needs. If the research structure is to survive with Congressional support, it must occupy itself with the questions of the moment. There is no time to worry about the farmer's grandchild.

Congressional interest in research projects introduces other trouble, too, such as making it much easier to go on spending money on something established than to institute anything new. Hence, the scientists have to try to extend already established researches and bend them around new needs, often getting only half as far as they should. And they sometimes continue researches that might have been stopped years ago at no great loss. This situation is particularly true where experiment stations have been set up. The presence of these stations, some of them very small, becomes important in the congressman's constituency. The Department of Agriculture budget supports several such stations which offer little but vain hope of ever serving any purpose that cannot be served more effectively elsewhere. Once in a while Congress gets too excited, or its prodders do, about some question involving agricultural research. When this happens, the scientists have to occupy themselves for a year or two with attempts to spend short term money effectively. Then Congressional interest in that particular problem wanes. The men in the research organization are left with nothing but the knowledge that a few thousand dollars a year for ten years and freedom to attack basic problems would probably have solved the difficulty. That the final determining review of research basic to the country's agricultural future should be in the hands of Congress is logical only if Congress seeks and is guided by the advice of the best specialists in the field. The record doesn't suggest that Congress operates in this way. Even if it did, the system would remain inadequate until the major part of the research could be relieved of the compulsive uncertainty that it reflects from the single-year appropriation scheme.

The federal plant scientists have another big handicap. Like the rest of the government machine of these United States, the Department of Agriculture is a bureaucracy—too large a portion of its staff has to be concerned with the functioning of the machine. And the scientist has to spend too much of his time making reports, reports, and reports on his organization for research. The research he cannot find time to do. A man who is good enough to get along soon finds himself occupied most of the time with reports on and justification of the efforts of those behind him in the line of ascent.

The agricultural research efforts of the states are conducted by the state experiment stations, which are affiliated with the land grant colleges. These agencies have many of the same difficulties as the federal system. They are too often staffed with people of insufficient breadth—partly because for many years most of the state experiment stations could pay only very low salaries, partly because the state scientists are under pressure by local legislatures and populations to direct all their attention to matters of immediate concern in the locality. The logical field of effort for such experiment stations concerns problems that are more or less local, having to do with the particular crops of a given state and their behavior and management under the environmental conditions in that state. It is generally considered poor economy for the state experiment station to allow its staff members to devote time to problems not immediately related to the needs of the state's farms.

The Evolution of the Problem

Many state research scientists and federal scientists share one characteristic that is somewhat less readily

recognized than those we have been discussing, but that is perhaps the most important in an over-all consideration. This is their failure to foresee what might be called the evolution of agricultural problems and to prepare themselves to deal with the products of this evolution. When the great agricultural agencies of the nation were organized, the compelling problems, for the most part, had to do with handling crops in the field—determining the proper soil for a given crop, the best time of planting, the most productive spacing, and the most effective fertilizers. With the development of agricultural engineering, there were the added problems of building machines for planting, cultivating, and harvesting different kinds of crops under different growing conditions, and developing methods for the control of insect and disease enemies. The agricultural agencies were founded for attacks on such problems as these. The present status of American agriculture is a tremendous tribute to the achievements of the agricultural research scientists in meeting these problems and solving them effectively. Much remains to be done, but what is not clearly enough recognized is that by solving these problems so effectively these same scientists have opened up new frontiers of research, have pushed the research needs along to another stage.

We are now at a point where major agricultural advances can come only after we have gained much more knowledge concerning the fundamental biology of our crop plants. Improvement in field practices may be expected to give perhaps another ten percent in increased yields. It will result in saving some land and it will bring further lightening of the farmer's labors. This sort of progress must be continued, but major advances will come only if we supplement such investigations with enough basic biological research to enable us to begin to learn something more than superficial facts about how plants grow, how they reproduce, and what their relations to soil and climate are. The agencies which have been built up under civil service regulations are staffed almost entirely with the researchers to whom goes the great honor of having solved the first line problems. However, it follows from the very nature of these investigators' training and interests that they generally do not recognize or have an interest in the now basic biological problems. Hence, the agricultural research agencies are tending to neglect seriously the biological problems which are at the base of the next century's improvement in agriculture or even the next century's continuation of agriculture at its present highly productive level. They are neglecting these problems sometimes for lack of time to deal with them, often on the assumption that the colleges and universities will produce a generation of men better prepared to deal with them.

This last point needs examination in the light of the facts. The avowed purpose of most agricultural colleges is to train agriculturists. To this end their faculties are organized and their curricula developed; and to this end they are generally most successful. There is, however, much confusion between the requirements of agriculture and the requirements of agricultural research. Young men going through agricultural curricula are encouraged to stay on and work toward advanced degrees, and then to accept positions of responsibility in agricultural research. If they are to deal with the kind of problems which were the predominant ones in agricultural research during the last half century, they are generally adequately prepared. We shall need many more of them. But we also need a different type of investigator, a member of a group in step with the newer problems in the second line of research, with the fundamental biological and biochemical problems. It is a mistake to assume that a man trained in agriculture is qualified for this type of specialized agricultural research.

If the agricultural colleges must continue to devote their efforts mostly to the training of agriculturists, then where are the people capable of solving the more complex fundamental biological problems to come from? One might expect that colleges and universities which are less directly connected with agriculture should be in a position to train such people. Though they may be in such a position, too often they are not interested. Their botanists and zoologists tend, more or less purposely, to concentrate their efforts on investigations which have no bearing upon practical problems. Their chemists and physicists are busy responding to the demands of industry, which recognized long ago the need for continuing to harvest annual crops of fundamental researchers.

It behooves these colleges and universities to examine the thesis which guides their training of young men and women for productive lives. It behooves these institutions and the agricultural colleges to look further into the basic training requirements for agricultural research. It behooves a lot of professors to examine the relative worth of the problems with which they occupy themselves and a lot of practical-minded agriculturists to look far enough into the ivory towers to see whether anything in the way of useful knowledge may be written on the walls. Yale University and the University of Connecticut have recently established what may become a pattern for the training of agricultural research scientists. In a cooperative scheme, students are given two or three years' work in the fundamental biological sciences and directly allied fields at Yale and then they go to the University of Connecticut for a year or more to study application of the facts and techniques of these fundamental sci-

ences to the problems of agriculture. Although it would not seem adaptable under most circumstances, this scheme recognizes the key to the problem. Agricultural research in the commonly accepted sense is not enough. It must be expanded to include far more biological research. In the case of plants and their products, we must direct major rather than incidental efforts toward unraveling the facts of growth, development, and reproduction, and the interrelations of these with soil and climate. When enough such background has been accumulated, then agriculture's practical problems must be reviewed against it.

To gain this critical knowledge we need to make certain changes. Our federal research agencies must be relieved of the requirement to concentrate their efforts on sure-fire, immediately solvable problems, while their scientists defend Congressional mandates for fundamental research. They must expand their research to determine how the green plant combines carbon dioxide and water to produce carbohydrate and then transforms the carbohydrate to thousands of useful substances, and to attack a selected group of other fundamental problems. This calls for a revision of the setup, a new deal from civil service, new types of Congressional authorization and support—all matters of federal government concern. But it calls for other things, too. There must be leaders who can lead, men who can appraise trends in all the sciences and interpret their significance for agriculture, who can recognize the gaps that stop progress, and furnish the individual scientists with charts for action. Such men are likely to come only after a renaissance in teaching and training has directed students in basic biology, chemistry, and physics toward agricultural problems. This calls for reorientation by the colleges and universities with direct or indirect interest in the field, closer cooperation between pure and applied science groups, and the development of programs which will train much more highly and educate more broadly at the same time.

The Significance of Meiosis in *Allomyces*

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TWENTY YEARS AGO Hans Kniep (*7*) described in the water mold *Allomyces* a life cycle and a type of sexuality which were previously unknown in the fungi. The cycle, which now serves to distinguish the subgenus *Euallomyces* (*3*), is outlined in Fig. 1. Sexual reproduction is accomplished

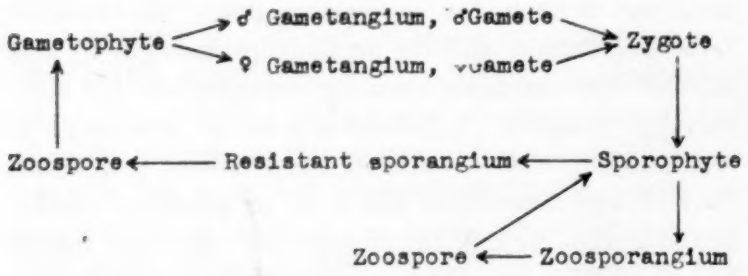

FIG. 1. Life cycle of *Euallomyces*.

by fusion of a small, motile, pigmented male gamete with a somewhat larger, motile, unpigmented female gamete. A year later, having discovered that the nuclei in sporophytic hyphae had about twice the volume of those in gametophytic hyphae, Kniep (*8*), postulated that meiosis occurs in the resistant sporangia of *Euallomyces* and that there is an alternation of haploid gametophytes and isomorphic, diploid sporophytes. Similar studies subsequently led Sörgel (*12, 13*) to accept this concept, and Emerson (*3*) presented genetic evidence, obtained from interspecific crosses, which gave strong indirect support for Kniep's interpretation of the life cycle. Although Hatch (*4*) had made a detailed cytological study of dividing nuclei in zygotes of *Euallomyces* and concluded that meiosis occurred at zygote germination, he later (*5, 6*) denied the validity of his own interpretations and accepted Kniep's hypothesis.

Kniep himself was quick to recognize the unusual possibilities which *Allomyces* presents for experimental investigations of sexuality and reproductive behavior, and subsequent studies by others amply testify to the keenness of his insight. A variety of basic researches using this phycomycete to investigate phenomena of apomixis, sex determination, irradiation action, nutrition, metabolism, and the physiology of spore-maturation, -dormancy, and -germination are under way in a number of laboratories in the United States at the present time. Nearly all of this work

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is directly or indirectly related to chromosome behavior, and the urgent need for exact knowledge of the meiotic divisions has become increasingly apparent. By using the well-known aceto-orcein smear technique, the writers have recently succeeded in completing a critical cytological study of meiosis in *Allomyces*. It is the purpose of the present note, therefore, to give a brief report of the observations which have been made and to emphasize the bearing of these findings on the future use of *Allomyces* in various phases of biological research and teaching.

Resistant sporangia formed by sporophytic thalli grown on slants of yeast-starch agar (*3*) ordinarily become capable of germination three to six weeks after their formation. At this time each sporangium contains about a dozen expanded, diploid nuclei in an advanced prophase stage. These sporangia are fully mature and, if air dried, they will remain viable and their nuclei will remain in prophase without any further detectable change for periods up to at least ten years. When mature resistant sporangia are taken directly from moist agar cultures and placed in water at 20° to 25° C, they form and release spores in 100 to 130 minutes. During this short interval the two meiotic nuclear divisions occur and are immediately followed by cleavage of the cytoplasm and organization of the zoospores. Each of these zoospores is haploid and normally uninucleate, and it has been determined that there are four times the number of zoospores per resistant sporangium as there were diploid nuclei at the start. Hence no haploid mitoses occur in the resistant sporangia. All of the nuclei in a given sporangium undergo meiosis almost simultaneously, and it has been possible to examine many preparations of each of the critical stages in both divisions. Careful analysis of metaphase and early anaphase figures of meiosis I has shown that the haploid chromosome number is probably 7 in *A. arbusculus* and 14 in *A. javanicus* var. *macrogynus*. Preliminary observations of meiosis in the F_1 resistant sporangia from crosses between these two species have revealed pairing of some of the 21 chromosomes and random distribution of the remainder.

The life cycle of *Allomyces cystogenus*, which is in the subgenus *Cystogenes* (*3*), differs strikingly from that of *Euallomyces*. As is shown in Fig. 2, there is no multicellular sexual plant, the zoospores from resistant sporangia becoming transformed directly into gametangial cysts, which release isogametes (*14*). Although various possibilities had been suggested, nothing was known about the place of meiosis in this cycle until it was discovered in the present study. Aceto-orcein smears of resistant sporangia of this species revealed a sequence of events exactly similar to those just described. There are two meiotic nuclear divisions which just precede spore formation. In the entire life cycle, therefore, there is but one mitotic division of haploid nuclei, and this occurs during gametogenesis in the cysts. The haploid chromosome number of *A. cystogenus* appears to be 7.

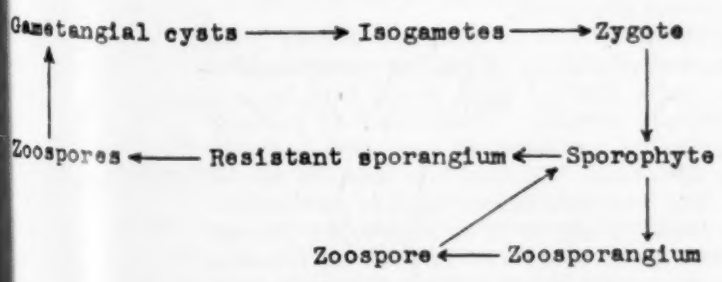

FIG. 2. Life cycle of *Cystogenes*.

The writers wish to focus attention on the relation of the foregoing observations to certain matters of general significance. First, it has been tacitly accepted for more than twenty years that meiosis in the common aquatic phycomycetes such as *Saprolegnia* occurs in the later stages of development of the fertilized egg, that is to say oospore. An examination of the literature, however, shows that virtually all of the evidence is negative, depending on studies which demonstrate that the nuclear divisions which precede gametogenesis are not meiotic. Indeed, although meiotic nuclei have been seen in several genera (*1, 2, 9, 10*), the present investigation appears to be the first critical step-by-step study of meiosis in an aquatic phycomycete. The successful application of the aceto-orcein technique to *Allomyces* indicates that renewed efforts to examine the meiotic processes of other phycomycetes should be revealing, despite the generally small size of the nuclei in these fungi.

Second, it should be understood that the conclusive demonstration of meiosis in the resistant sporangia of *Allomyces* places it, as Kniep (*8*) has already indicated, in the unique category of a filamentous fungus in which there are true diploid nuclei in the vegetative hyphae. The picture becomes even more striking when it is realized that mitoses are diploid throughout the entire cycle of *A. cystogenus*, with the exception of the single haploid division involved in gametogenesis. This nearly complete suppression of the haplophase is remarkably similar to that which occurs in some of the siphonaceous green algae. It is also interesting to note the close parallelism between present concepts of haploidy in the water molds and the situation which existed about twenty years ago, when Schussnig (*11*) and others were demonstrating the fallacy in the belief, generally held at that time, that all green algae are haplonts.

Third, it is obvious that knowledge of meiosis and specific chromosome numbers in *Allomyces* is going to facilitate greatly the planning and interpretation

of genetic studies with this fungus. The feasibility of bringing large numbers of nuclei into meiosis at will on a definite time schedule will be of significance in future irradiation work. Self-fertility of the hermaphroditic sexual plants of *Euallomyces* makes possible in a single karyogamy the production of homozygosity in diploids, while normal dominance relations can be studied in heterozygous diploids.

Fourth, with 7 and 14 as basic haploid numbers, and with a considerable degree of interspecific compatibility, there is a very promising outlook for cytotaxonomic research, a field in which fungi have played little or no part hitherto.

Fifth, establishing the place of meiosis in *Euallomyces* has finally put this water mold in an exceptional position as a laboratory organism with which to demonstrate with diagrammatic simplicity each of the basic structures and processes involved in a generalized, sexual reproductive cycle. Sexual and asexual plants can be grown separately on ordinary laboratory media (*3*) and will produce their respective reproductive structures in profusion without special treatment. The release of gametes, haploid zoospores, or diploid zoospores can be induced at will in a given time simply by placing gametangia or sporangia in water. Arranged in terminal pairs, the male and female gametangia are distinguished by a striking color difference, the females being unpigmented, the males brightly colored with carotene. The difference in size and pigmentation of the gametes themselves makes it possible to identify them after their emergence, and the actual process of syngamy can be followed under the ordinary compound microscopes provided in an elementary laboratory. Surely few other organisms combine so fully and vividly those precise features required for an introduction to concepts and patterns of reproductive mechanisms in living things.

With its ready growth of either diploid or haploid phase in pure culture, its uninucleate motile gametes and zoospores, its colorfully differentiated gametangia, its hermaphroditism and self-fertility, its distinctive resistant sporangia which can so conveniently be used to perpetuate stock strains, and its uniform rapid response to transfer from nutrient agar to water, *Allomyces* is an organism with outstanding possibilities in the fields of biological research and teaching.

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AAAS Recommendation on Atomic Energy Act Amendments

The following memorandum, adopted by the AAAS Executive Committee at its meeting July 7, 1949, was sent to the McMahon Committee of the Senate:

The Executive Committee of the American Association for the Advancement of Science urges upon your Committee and the Congress the exercise of searching scrutiny and objective consideration of any amendments to or revisions of the Atomic Energy Act which would affect seriously if not disastrously the nation's progress in research. Such progress has resulted in the past less from great expenditures of money than from freedom of publication and of communication among scientists. Research has little potential value until its results are public property.

This Committee views with grave misgivings the proposal reported in the press that the export of radioisotopes for research by scientists in other countries be prohibited by statute, on the ground that such research might be applicable to military developments, and its publication might be detrimental to our national interest.

Any basic research has potential applicability to developments of all kinds, including military. This must be regarded as a small "calculated risk." The radioisotopes available to foreign scientists are of a kind not used in weapons, and their utility is preponderantly in tracer studies and medical research. The likelihood of their being employed detrimentally to our interests is practically nonexistent, whereas the potential benefits to mankind are great.

If there is hazard to us in the publication of results obtained abroad with radioisotopes, there is also hazard in publication of results of similar research done in this country with radioisotopes; and the same kind of logic would lead to prohibition of such publication here as of shipments abroad. Prohibition of publication would be highly detrimental to our interests, for research can thrive only in an atmosphere of free interchange among scientists. Without it there is no research. In view of the indisputable fact that research is essential to our leadership, it would be a serious matter indeed to prohibit export of radioisotopes (except those fissionable materials usable in weapons), as it would also to prohibit freedom of publication of results of research in this country, in which radioisotopes are used.

TECHNICAL PAPERS

Nitrogen Mustard Inactivation of the Cytoplasmic Factor Kappa, in *Paramecium aurelia*, Variety 4[1]

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The growing importance of chemical mutagens and their use in experimental genetics make a study of their effects on cytoplasmic factors desirable. The present work is concerned with the effects of the nitrogen mustard, methyl bis(ββ′-dichloroethyl)amine, on the cytoplasmic factor kappa, in *Paramecium aurelia*, variety 4, stock 51.

The first experiments were designed to test the effect of nitrogen mustard on killer animals growing normally at 27° C. In experiment 1, 2 ml of a freshly prepared buffered solution (phosphate, pH 6) of the hydrochloride salt of nitrogen mustard in a concentration of 0.19 mg/ml was added to a Columbia dish containing approximately 1000 killer animals in a small drop of culture fluid. Samples of up to 50 animals were washed and the animals were isolated individually and allowed to multiply for several days at maximum fission rate. At the end of this growth period, the cultures were tested for the presence of animals sensitive to the action of paramecin, the antibiotic which is produced by killer animals and which is responsible for their killing action. The method for the determination of the character of a culture is as follows. Samples of the culture in question are mixed separately with known killer animals and with known sensitive animals. If killing occurs in the mixture with killer animals, the culture contains sensitive animals. If killing occurs in the mixture with sensitive animals, the culture contains killer animals. If killing occurs in both mixtures, both sensitive and killer animals are present in the culture. In this case, the culture in question would show autolethality, i.e., the sensitive animals in the clone would be killed by the killer animals present. This method for testing the character of a culture was used throughout the study.

Experiment 2 was performed in the same manner as experiment 1. The exposing concentration was 0.434 mg/ml and different time intervals for exposure were used. Both experiments are summarized in Table 1.

It may be seen from this table that some of the killer animals were changed so that they gave rise to sensitive progeny. It will also be noted that, in general, the heavier the exposure to nitrogen mustard, the greater the proportion of clones containing sensitive animals and the greater the proportion of completely sensitive clones produced. Mortality also parallels the length of exposure. Killer animals not exposed to nitrogen mustard fail to produce sensitive progeny under similar conditions.

One interpretation of these data is that kappa has been inactivated in varying amounts with the different exposures. The production of sensitive animals, therefore, means that the number of kappa particles remaining in an animal is small enough to allow the animals to become sensitive to paramecin action (*2*). Animals which, at fission, receive at least one particle of kappa are able to give rise to clones which contain killer animals because kappa, when not affected by nitrogen mustard, can multiply more rapidly than the animals under the conditions of the experiments. Animals receiving no kappa are unable to initiate kappa formation and so remain sensitive (*2, 5, 6*). That this is the correct interpretation is shown by the experiments described here.

TABLE 1

EFFECT OF NITROGEN MUSTARD ON ANIMALS GROWN AT 27° C

Expt.	Length of exposure (hr)	No. isolated	Total No. tested*	Fraction of total tested: containing any sensitive animals	Fraction of total tested: containing only sensitive animals†
1	0.167	40	40	0.05	0.00
1	0.250	46	46	0.02	0.00
1	0.500	49	49	0.43	0.00
1	1.000	42	42	0.43	0.00
1	2.000	50	50	0.56	0.00
1	4.000	49	49	0.12	0.00
1	8.000	49	19	0.68	0.32
2	1.000	39	38	0.95	0.03
2	2.250	38	38	0.97	0.34
2	4.000	39	31	1.00	0.32
2	6.250	38	16	1.00	0.75
2	24.000	40	1	1.00	0.00

* Differences in the number isolated and the number tested are due to mortality.

† Clones containing only sensitive animals do not regain killing ability even when grown at one fission per day for three weeks. These clones, then, may be considered as having been derived from animals in which kappa was completely destroyed.

The expansion technique (Sonneborn, unpublished) allows the mean kappa particle number of the progeny of a killer animal to be reduced to any desired level. This technique involves growing the animals at 33.8° C, at which temperature the number of kappa particles fails to increase, and the animals undergo rapid fission, thus decreasing the number of kappa particles within the animals at each fission. The period of rapid fission at 33.8° C is followed by a period of slow fission rate at 27° C,

[1] This investigation was supported by Indiana University and by Rockefeller Foundation grants in aid of research on protozoan genetics at Indiana University. The nitrogen mustard was supplied by Dr. Richard A. Ormsbee, of the Sloan-Kettering Institute for Cancer Research.

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at which temperature kappa can multiply faster than the animals. Those which contain at least one particle of kappa produce progeny with the full number and so yield killer clones. Those containing no particles remain sensitive and yield sensitive clones. This provides a means for calculating the number of kappa particles present in the original killer animal. The number of kappa particles present, on the average, in the progeny of a killer animal subjected to the expansion technique may be calculated by the approximate relation $P_0 = e^{-m}$, where P_0 is the proportion of individuals with no particles, m is the mean number of kappa particles per animal, and e is the base of natural logarithms (*1*).

If inactivation of kappa particles has occurred in animals exposed to nitrogen mustard, one can measure the extent to which this has occurred by comparing the mean number of particles in the progeny of exposed animals to that in the progeny of normal killer animals subjected to the expansion technique.

This comparison between animals exposed to nitrogen mustard (0.20 mg/ml) and animals exposed to buffer alone (controls) for a period of 7 min was made in experiment 3. A fresh solution of nitrogen mustard hydrochloride in phosphate buffer (pH 6) was made up twice as concentrated as was desired for the exposure. One drop of this solution was used immediately by adding it to an equal-sized drop of buffer containing the animals in a depression slide. The two drops were mixed quickly by agitating the slide, and after 7 min the animals were removed, washed three times in fresh culture fluid, isolated, and placed at 33.8° C for rapid fission. The exposed animals underwent seven fissions, and the controls underwent eight fissions. The results of carrying out these procedures on seven exposed and nine control animals from the same clone are given in Table 2.

TABLE 2

EFFECT OF NITROGEN MUSTARD ON THE NUMBER OF KAPPA PARTICLES IN KILLER ANIMALS

Animal No.	No. isolated and tested	Fraction sensitive	Mean No. of kappa particles
Exposed			
1	127	0.724	0.323
2	118	0.797	0.227
3	115	0.765	0.267
4	127	0.874	0.135
5	118	0.780	0.248
6	118	0.797	0.227
7	117	0.667	0.405
			Avg 0.261
Control			
1	233	0.236	1.44
2	99	0.222	1.42
3	98	0.092	2.39
4	98	0.143	1.95
5	98	0.122	2.10
6	94	0.106	2.24
7	27	0.148	1.91
8	29	0.138	1.98
9	24	0.167	1.79
			Avg 1.91

From the proportions of sensitive animals produced in the two series shown in the table, one may calculate that the average mean particle number for the progeny of the control animals was 1.91, whereas that for the exposed animals was 0.261. Since the controls went through one more fission than the exposed animals, the control value of 1.91 particles per animal must be doubled to make the value comparable to that found in the animals exposed to the action of nitrogen mustard. The percentage of kappa remaining after exposure is then $\frac{.261 \times 100}{2 \times 1.91} = 6.8\%$.

Experiments carried out using different lengths of exposure to nitrogen mustard indicate that the longer the exposure, the greater the percentage of kappa particles inactivated.

This procedure cannot be placed on an accurate quantitative basis at the present time because the effects of temperature alone on kappa have not been thoroughly investigated. There should be a way to correct for the effects of temperature after these have been studied in detail.

The results reported here were confirmed with the aid of the light microscope, using the methods of Preer (*3*, *4*). The cytoplasm of killer animals contains Feulgen-positive particles which are not found in sensitive animals, and which parallel the behavior of kappa. The numbers of these particles exhibited by animals exposed to nitrogen mustard were much smaller than the numbers found in untreated animals. There then seems little doubt but that the staining reaction associated with kappa and its activity as regards the killer character are destroyed by the action of nitrogen mustard.

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A New Method for the Study of Submicroscopic Spaces

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Proceeding with their research on the optical properties of osseous substance, Dallemagne and Melon (*2*) attempted to obtain diagram curves of structural double refraction of the organic constituent of bone. Since the results, which will be published, have shown anomalies in the general aspect of the diagrams, we have been looking for a different technique for scanning the submicroscopic

FIG. 1. Mineralized bone embedded in methyl methacrylate (polarized light).

FIG. 2. The same material after treatment by hydrochloric acid, i.e., the "bone ghost" (polarized light).

spaces occupied by the organic substance in bones.

Slices of dry bone (200–300 μ thick) were sawed, then boiled (290° C) in anhydrous glycerol containing 6% KOH, until no trace of organic substance remained, according to Gabriel's method (*3*).

The bone slices were then carefully washed with distilled water, dried, and embedded in methyl methacrylate according to the technique of Coudert and Baud (*1*).

The preparations immersed in the pure monomer were placed in a vacuum until no more air bubbles escaped. Afterwards the preparations were introduced into slightly polymerized methacrylate and left for 12–24 hr at 56° C in airtight containers in the presence of benzoyl peroxide; the polymerization of the plastic was then accomplished.

The Plexiglas block was abraded on both faces by a grindstone until the embedded specimen reached the surface; it was then polished.

Examined under the microscope, both in ordinary and polarized light, the object showed, of course, the usual picture of mineralized osseous substance. Between crossed Nicols, it showed a retardation corresponding to impregnation with a medium having a refractive index of 1.490, which is the refractive index of polymerized methyl methacrylate (Fig. 1). The specimen was then immersed in a solution of normal hydrochloric acid and kept there until no more gas escaped, thus showing that all the carbonate was converted into chloride and that the mineral substance of bone had completely disappeared.

In fact, after this treatment, we cannot detect traces of bone when the plastic block is dissolved in acetone. However, such preparations, free from the initially embedded mineral substance, still look like ordinary osseous slices. Actually, this appearance is due solely to methyl methacrylate deposited in the submicroscopic spaces of mineralized bone. Thus, we have a "bone ghost."

In polarized light, we observe a picture topographically identical with the one obtained before hydrochloric destruction of the mineral substance (Fig. 2).

On the one hand, methacrylate becomes set in the submicroscopic spaces originally occupied by the organic constituents which are liberated through deproteinization; on the other hand, the mineral particles removed by hydrochloric acid leave free spaces that can thereafter be filled with liquids endowed with a different refractive index.

By means of such casts, it is possible to establish structural double refraction curves without fear of getting results spoiled by accidental influences (swelling, alteration by reagents, etc.).

The results of our optical research on "bone ghosts" will be published in detail elsewhere.

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Levels of Adaptation and Brightness Changes During Color Adaptation

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In an article which appeared recently in *Science* (*3*), Jozef Cohen has restated the major generalizations which summarize his data on color adaptation (*2*). Intensity, according to these generalizations, increases during color adaptation.[1] The increase, while independent of the hue of the stimulus, is dependent on its saturation and intensity. The general conclusion concerning the change in intensity stands in direct contradiction to the findings of a number of earlier investigators, who usually concluded that brightness (intensity) decreased rather than increased during the course of color adaptation (*1*, *6*, *7*, *8*).

Cohen is well aware of this major discrepancy. He is also aware of the lack of complete agreement among previous investigators concerning other aspects of color adaptation, and he reports (*3*) an experiment designed to determine the source of the discrepancies. Most investigators have described their stimuli only in terms of color appearance. Cohen, therefore, tested the hypothesis that color adaptation of the eye is a function of the spectral composition of the stimulus and not of its tristimulus values (or appearance). Although the stimuli used by different earlier investigators may be comparable as regards tristimulus values, unrecognized differences in the physical compositions of various stimuli might have led to different adaptive effects. Cohen's experimental results were, however, negative. Stimuli of different spectral distributions but of the same tristimulus values produced no appreciable differences in color adaptation. Although this finding is not without its own intrinsic interest, the discrepant results concerning brightness changes during color adaptation still remain unexplained.

An analysis of the experimental methods and procedures used by most investigators, including Cohen, suggests an alternative explanation in terms of differential adaptation effects. The specific aim in color adaptation studies of this sort is to determine the manner in which a visual sensory effect, produced by a test stimulus of fixed intensity (luminance) and spectral distribution, varies with continued exposure. The variation in brightness, for example, is measured by equating to the test field after various times of exposure a comparison stimulus of variable intensity which stimulates a spatially separate retinal area in either the same or the other eye. Changes in the matching intensity of the comparison field are then taken as measures of the adaptive effect in the test area.

Unfortunately, however, in studies of this sort the responses evoked in the comparison area are, like those of the test area, also conditioned by initial and subsequent states of adaptation. Even if one were to assume, as Cohen does in his experiments, that a static or equilibrium state can be maintained in the comparison area during the course of the measurements, the initial level of adaptation of the comparison area still exerts a decisive influence upon the result. Visual comparisons between two discretely stimulated retinal areas depend not only on physical stimuli but upon the respective adaptation levels of both areas. Craik, in an extensive study of the effects of adaptation on brightness, has given a striking experimental demonstration of the extent of this dependence: "... an illumination of 3 e.f.c. presented to the dark-adapted left eye is judged subjectively equal to the adapted brightness of 15,000 or 75,000 e.f.c. presented to the right eye" (*5*, p. 239).

Since brightness equations made during the course of color adaptation depend on the relative adaptation levels of the visual test area whose adaptive changes are being measured and the visual comparison area which is used to measure these changes, the conflicting results which have been reported may very probably be due to different relations between these two levels in different experiments.

An examination of experiments in which a decrease in brightness of the test area during color adaptation is reported shows that the retinal comparison area is adapted to a lower illuminance level than the test area. Except for flash exposures necessary to make a color match to the test field, the comparison area is unstimulated during adaptation to the test color. The comparison area is in effect dark-adapting. Since its sensitivity, relative to that of the test area, is increasing with time, it is therefore to be expected that less and less light will be required for an equation to the test brightness.

It follows directly that opposite effects should occur if the adaptive levels and consequently the relative sensitivities of the two areas are reversed. With the adapting illuminance of the comparison area higher than that of the test patch, the intensity required for a match would be expected to increase during adaptation, as it did in Cohen's experiments. Were this one of his experimental conditions, the unexpected direction which his results took might readily be explained. In his experiments, in contrast to other investigations, the retinal comparison area was continuously stimulated during the adapting period by what he calls a neutral "gray of medium brightness" (*2*, p. 100). Unfortunately, the intensity level is not specified. It is asserted simply that "the patch upon which the match was made underwent little

[1] Cohen uses the word *intensity* to specify both the psychological and physical dimensions. His conclusions, however, refer specifically to measured physical variations in a matching stimulus during color adaptation. Given certain known psychophysical relations between stimulus intensity and sensation, such measurements are frequently interpreted as indicative of changes in the psychological dimension, and to describe them the word *brightness*, rather than intensity, is more commonly used. For specifying the stimulus, *luminance* is preferable to *intensity*. Intensity is restricted to the specification of point sources, while luminance, measured in lamberts or foot lamberts, specifies the intensity per unit of projected area of source. (See Colorimetry Report, Reference *4*.) Since the older work with which the present paper is concerned has been reported and discussed in terms of "stimulus intensity," this usage is retained in the present discussion to avoid confusion.

if any, change, i.e., the retinal processes were in equilibrium'' (*2*, p. 109). In the absence of precise specification of the adaptation levels of the test and comparison areas, the proposed explanation lacks experimental confirmation. The following experiments are intended to provide such confirmation.

The apparatus used in these experiments was a form of monochromatic colorimeter. The field sizes and positions, pre-exposure times, and adaptation controls were identical with those used in the Cohen investigation (see *2* for details). Observations were binocular, with both the test and comparison fields viewed by each of the two eyes. Judgments, however, were restricted to one psychological dimension—namely, brightness. To test the effects on brightness equations of differences in the relative adaptation levels of the test and comparison areas, three separate color adaptation experiments were performed. The stimulus intensity of the test color was the same in all three experiments. The adapting intensity of the neutral comparison field, while always within a range which might be considered of medium brightness, was less than, equal to, or greater than that of the test field in the separate experiments. The general result for two observers is shown in Fig. 1.

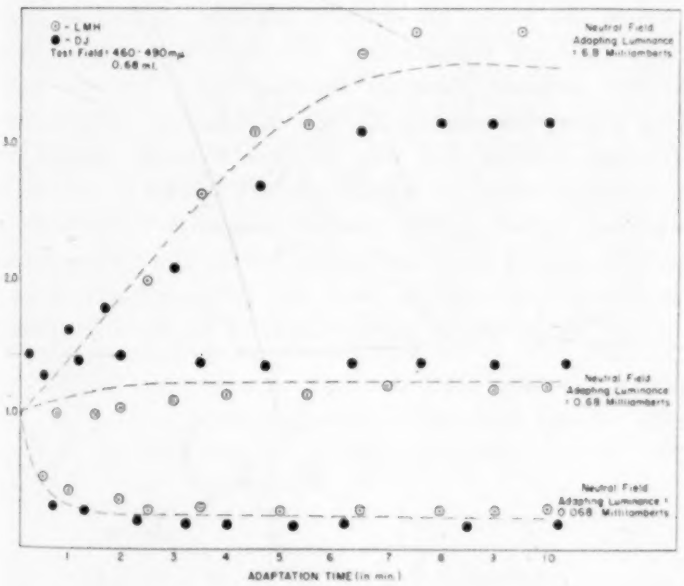

FIG. 1.

The lower curve in Fig. 1 indicates that the intensity of the comparison field necessary for a brightness equation to a test field of fixed stimulus intensity decreases with an increase in the time of adaptation. For this result, the achromatic adapting illuminance of the retinal comparison area was about one-tenth that of the colored test area. In the second experiment, on the other hand, the comparison area was adapted to approximately the same illuminance level as the test area. In this case, the curve shows that there was very little change in the matching intensity during the course of the adaptation period. Finally, the upper curve shows the matching intensity to increase with an increase in adaptation time. Here, the adapting illuminance of the comparison area was about ten times that of the colored test area. The increase in intensity under these conditions is similar to the result obtained by Cohen. The presumption seems warranted, therefore, that Cohen's experimental conditions were also comparable, and that the adapting illuminance of his retinal comparison area was, in fact, higher than that of his test area.

The results shown in Fig. 1 have been confirmed for each of two observers for two hues and two levels of test color illuminance. In all cases, the direction taken by the intensity function obtained is specific and reproducible, and depends on the relation between the adaptation levels of the retinal test and comparison areas. Fig. 1 conclusively demonstrates that the increase in intensity reported by Cohen cannot be accepted as a general law of color adaptation, any more than the decrease reported by other investigators can be so considered.

The curves presented in Fig. 1 are, in effect, equal brightness contours for different adapting levels, determined over specified periods of time and for particular experimental conditions. They reconcile the apparently discrepant results which have been interpreted as representing the manner in which brightness varies during the course of color adaptation. Actually, these curves cannot be taken to represent the manner in which the brightness attribute of a color varies with adaptation. They are functions which result from the *variations of two sensitivities*, variations in the test and comparison areas. The test sensitivity cannot be measured independently unless it is guaranteed that the comparison area is maintained at a constant level which is the same as the initial level of the test area. That a constant state of sensitivity can be maintained in the measuring area of the retina has long been doubted. To quote Almack, ''That this can be accomplished under such unusual circumstances as are imposed in an adaptation series is highly improbable . . . the choice of a method of equivalents is, we believe, an unfortunate one for rating loss of sensitivity by adaptation'' (*1*, p. 33). Further research alone will determine the validity of this point of view. It must again be emphasized, however, that even if the measuring area is demonstrated to be constant in sensitivity, results would still represent only relative changes from the specified initial sensitivity of the test area to subsequent states produced by adaptation to the retinal test illumination. Nevertheless, with complete specification of the experimental conditions, and with full awareness that results obtained in this manner represent events of a relational nature only, they, like any other measurable phenomena of color vision, provide much useful information. They require and will ultimately receive a necessary explanation in terms of underlying physiological processes.

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Measurement of Ionizing Radiations *in Vivo*[1]

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It is well known that the intensity of roentgen rays can be quantitatively measured by using a photomultiplier tube to pick up the light from a fluorescent screen exposed to these rays. It has occurred to various investigators that the light from the screen can be transmitted by various types of optical systems to the photocell. The optical system does not have to produce an image; in its simplest form it is an internally reflecting tube or light pipe. For roentgen-ray intensities used in therapy the amplification of the photomultiplier tube is far greater than is necessary for convenient measurement. This leeway suggested the possibility that the system would still be sensitive enough if the light pipe had a very small diameter and the fluorescent screen occupied a very small area at the end of the light pipe. It seemed possible that the light from the screen could be piped through a hypodermic needle or through small diameter Lucite or quartz rods for insertion in body cavities. Both these possibilities have been realized, permitting direct measurements of roentgen-ray depth doses *in vivo*. The first system tried used Lucite rods of ¼-in. and ⅛-in. diameter and about 10 in. long. With the view in mind that ultimately these rods could be used to transmit separate scintillation pulses instead of a steady light level, the ends of the rods were turned in a parabolic section as shown in Fig. 1; a small hole was drilled to the focus of the parabola and a very small amount of luminescent zinc sulfide phosphor was pushed into the bottom of the hole. The rods were then aluminized by evaporation and coated with a black varnish. The total internal reflection from an air interface could not be depended upon, since in use the rods are frequently immersed in water. This arrangement would give a much smaller number of scintillations than could be had by cutting the end of the rod square and coating it with a layer of fluorescent crystals, but much more light from a single scintillation could be collected and reflected along the inside of the rod. This arrangement still had plenty of reserve sensitivity for roentgen-ray dose measurements, thus permitting a very fine-grained resolution of isodose curves to be determined.

To illustrate the potentialities of this arrangement the isodose curves were plotted in a pail of water subjected to a beam of 65-kv roentgen rays. Fig. 2 shows the arrangement used. As the ⅛-in. rod still had plenty of reserve sensitivity, work was started to pipe light from a speck of fluorescent material through a hypodermic needle to the photocell. This worked very well with a No. 15 needle. An internally polished needle gave more than twice the response of an unpolished one. The needles were polished internally by running them back and forth on a stretched copper wire charged with polishing compound. In these tests the needles were not Lucite-filled. A few grains of zinc cadmium sulfide phosphor (Patterson type B) were stuck on a tiny tapered Lucite plug, which was forced in a short distance up from the point of the needle. The needle could be inserted into an animal in the usual manner. It was inserted into the abdomen of an anesthesized rat to measure the abdominal depth dose for 60-kv roentgen rays. The dose measured in the abdomen in this case was found to be about 60% of the surface dose. The wall of the needle absorbed an undetermined amount of radiation, but this had no effect on the relative readings or on the ability of the device to be calibrated for any particular voltage quality of radiation. The luminosity of the screen per r-unit per minute depends on voltage quality and falls off considerably for radium γ -rays. A low atomic number phosphor showing only Compton scattering absorption should have a calibration independent of voltage quality but will produce a much smaller total luminosity than the highly efficient known phosphors containing higher atomic number elements. However, zinc sulfide should be considerably better than zinc cadmium sulfide in this respect, since for voltages higher than the 100-kv range the K energy level of Zn is only about 9.5

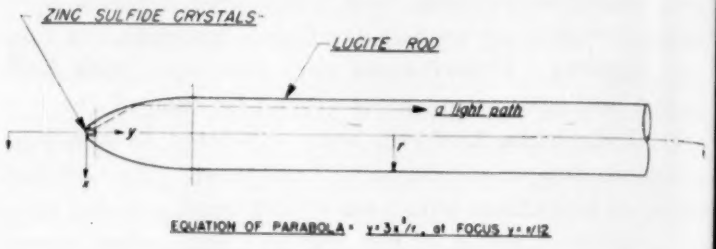

FIG. 1.

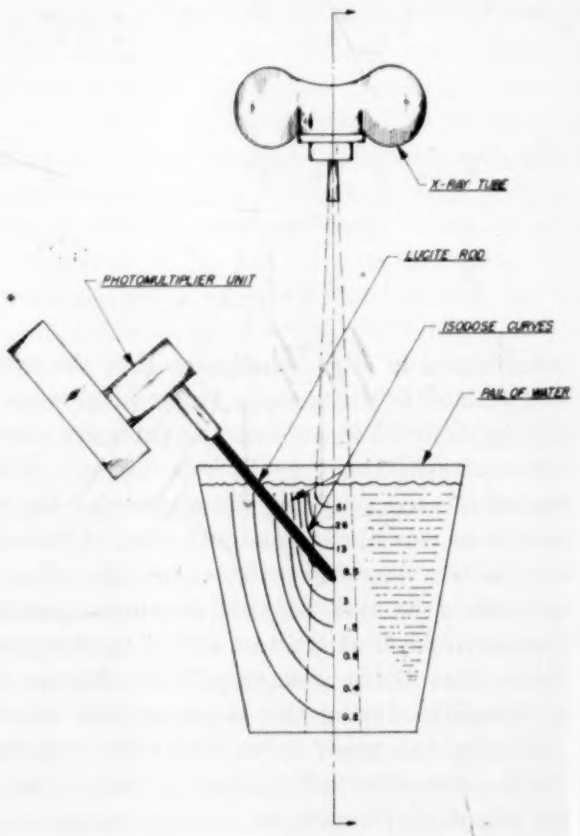

FIG. 2.

[1] This paper is based on work performed under Contract No. AT-04-1-GEN-12 between the Atomic Energy Commission and the University of California at Los Angeles.

kv. Although the photomultiplier tube (1P21) is more sensitive to the blue light from zinc sulfide than from the yellow-green light from zinc cadmium sulfide, the latter gave more response at 60 to 80 kv, presumably because of its greater absorption.

The experiments with the long Lucite rods were performed without any amplifier added to the photomultiplier tube. An amplifier of current gain of about 10^5 was constructed and used to observe the alpha particle pulses. This amplifier was not a d-c amplifier, so that it could not be used for measuring a steady luminosity level unless a mechanical chopper were used to interrupt the light entering the photomultiplier. However, it was easy to observe the half-wave pulses of X-rays from self-rectified X-ray tubes, even at extremely low levels. The intrinsic noise level, mostly due to shot effect from the thermionic emission of the photocathode of the photomultiplier tube, was easily observable with the full gain of the added amplifier. The extra gain is necessary to pick up alpha particle scintillations through the hypodermic needles and scintillations caused by beta rays.

Alpha particle scintillations on the zinc cadmium sulfide phosphor were so high as compared with the noise level that it was fairly easy to observe scintillations through the polished No. 15 needle. Further work is in progress to improve this technique and investigate the possibilities of measuring the local concentration of beta activity of radioisotopes *in vivo*. It has been found possible to turn Lucite rods of sufficiently small diameter to slide inside a No. 15 needle. These are coated with evaporated aluminum and a few crystals of fluorescent material are stuck on one end. The method of insertion is to slide them into the needle so that the screen end is slightly retracted from the beveled end of the needle. The needle is inserted to proper depth and then the Lucite rod is pushed on so that the screen end slightly protrudes into the tissue. One-mm-diam optically clear quartz rods gave even better results than the Lucite, but they have the disadvantage of being fragile.

Concerning the Specificity of Chicken Pancreas Conjugase[1]

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In a previous paper of this series (*1*), the conclusion was reached that chicken pancreas conjugase should be classified as γ-glutamic acid carboxypeptidase. The minimum requirements for the length of the peptide chain of the substrate could not have been established at that time. While it was suggested that pteroyl-γ-diglutamate was the end product of the reaction, the possibility of premature termination of the reaction, due to inhibition by the products formed, was not excluded.

We were able to study some recently synthesized derivatives of pteroylglutamic acid.[3] The methods used were the same as those previously described (*1*). The results are summarized below:

Substrate	*Equivalents of glutamic acid recovered*
Pteroyl-α-diglutamate	0 (or traces)
Pteroyl-γ-diglutamate	0 (or traces)
Pteroyl-α-α-triglutamate	0.0–0.2
Pteroyl-γ,γ-triglutamate	0.4–1.0
p-Aminobenzoyl-γ-γ-triglutamate	0.5–1.0

Since pteroyl-γ-diglutamate was not attacked by the enzyme, it represents the end product of digestion of the substrate by chicken pancreas conjugase. These data confirm our previous hypothesis and are in agreement with the findings of J. J. Pfiffner[4] who isolated the diglutamate from a digest of pteroylheptaglutamate with this enzyme. The minimum requirement for the number of glutamic acid residues for the substrate was, therefore, established at *three*.

In three out of five experiments with α,α-triglutamate, free glutamic acid was recovered. But the speed of hydrolysis of the alpha linkage was less than one-half that for the gamma linkage. Contrary to our previous conclusion, it appears that the chicken pancreas conjugase hydrolyzes the gamma linkage *preferentially*, but not *specifically*. The final decision must be withheld, however, until a purer enzyme is available.

The rate of liberation of the microbiologically active substance at various stages of purification of the enzyme was also studied. The ratio of the units of activity found with heptaglutamate[5] as substrate divided by the units found with triglutamate varied from 1:2 to 1:8, with an average value of 1:4.2. The calculated value for the diglutamate as the end product of the reaction is 1:5; for the monoglutamate, 1:3. In spite of considerable variation in experimental results, there was no tendency for a change in this ratio with increasing purity of the enzyme. This indicates that the liberation of microbiologically active substance from heptaglutamate is achieved by a single chicken pancreas conjugase, in contrast to the hog kidney conjugase, which has two components (*2*).

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[1] Aided by grants from the Donner Foundation, Inc., the Division of Research Grants and Fellowships of the National Institutes of Health, U. S. Public Health Service, and the John and Mary R. Markle Foundation.

[2] On leave of absence from the Agricultural Institute, Pulawy, Poland.

[3] Obtained through the courtesy of E. L. R. Stokstad, Lederle Laboratories Division, American Cyanamid Company.

[4] Discussion at the Federation Meeting, Atlantic City, 1948.

[5] Heptaglutamate was obtained through the courtesy of Dr. J. J. Pfiffner, Parke, Davis and Company.

The Site of Action of Some Chemical Agents in Diminishing Normal and Excessive Muscle Tension[1, 2]

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The ability to reduce or to efface normal muscle tension or to lessen or abolish states of increased tension in muscle by the administration of drugs not only promises new knowledge concerning neuromuscular mechanisms, but also offers the possibility of aid to those suffering from disorders in which excessive muscular tension is a prominent and disabling feature. Furthermore, the determination of the exact site of action of chemical compounds on central neurons may ultimately lead to the successful utilization of appropriate substances in a number of disorders and diseases of the nervous system. Among the chemical agents which are known to possess the property of reducing muscle tension, curare and its derivatives, especially D-tubocurarine, the erythroidine compounds, and myanesin have received the most consideration.

The relaxing effect of curare on muscle tension is generally considered to be due to blockade of the motor nerve endings, and the degree of relaxation is thought to be in direct proportion to the number of motor nerve endings affected. Thus, the term curare-like action when applied to a drug is generally understood to mean that the drug is able to effect a partial or complete motor end-plate block. It is an important and well-known observation that partial blocking of the motor nerve endings in the extremity muscles can be attained with curare before paralysis of the respiratory mechanism. This affords a basis for the administration of curare in order to increase muscular relaxation during anesthesia for surgery.

The effect of curare on muscle function has been extensively studied by Bremer and his associates (*4–6*). They found that small doses temporarily abolished or considerably diminished the extensor rigidity of decerebrate cats, without significantly affecting phasic responses. This differential effect was explained on the hypothesis that light curarization activated the latent fatigue which existed at the neuromuscular junctions undergoing continuous bombardment by the tonic impulses, and hence altered the endings in such a manner that those impulses producing the rigidity were more easily blocked than those concerned with phasic responses. All available evidence supports the view that curare exerts its relaxing effect on muscle by action at the motor nerve endings. However, it should be possible to bring about reduction in muscle tension from action on central neural mechanisms. Our investigations on the two types of compounds, other than curare, which have been most used for reducing muscle tension—namely erythroidine and myanesin—have revealed the fact that in the case of these drugs a central action exists.

The erythroidine alkaloids were first isolated by Folkers and Major (*7*) and the initial pharmacological studies were made by Unna, Kniazuk, and Greslin (*11*), and Unna and Greslin (*12*). Of the Erythrina alkaloids isolated, both dihydro-β-erythroidine hydrochloride and β-erythroidine hydrochloride were found to possess a curare-like action, i.e., they abolished the contraction of striated muscle to excitation of its motor nerve fibers while the response to direct excitation of the muscle remained. In addition to confirming these findings of a curare-like action, we have found that the erythroidine compounds are able to exert a relaxing effect on muscle tension by central action. This effect is evident with doses considerably smaller than those producing complete end-plate blockade, but it becomes masked by larger doses which block transmission at the motor ending. In the normal nonanesthetized cat or monkey the administration of appropriate doses of either compound, orally or parenterally, produces muscular relaxation without abolishing spontaneous muscular movements or tendon reflexes. As the dose is increased, spontaneous movements disappear and greater relaxation of muscle tension is secured, but even with a considerable degree of muscular relaxation the tendon reflexes remain present, although reduced and easily fatigued, and the muscle contracts upon excitation of the muscle nerve. With still larger doses, the deep tendon reflexes are no longer elicitable, the animal becomes completely paralyzed, the muscle no longer responds to stimulation of its nerve, and paralysis of respiratory movements ensues. With the erythrodine alkaloids, the added amount of drug necessary to paralyze respiration after beginning muscular relaxation is large enough to afford a considerably greater margin of safety than in the case of D-tubocurarine and as regards the two erythroidine compounds, the margin of safety is greater with β-erythroidine hydrochloride than with dihydro-β-erythroidine hydrobromide.

The amount of erythroidine required to produce different degrees of erythroidinization varies considerably and significantly as regards cat and monkey. In the nonanesthetized cat the amount of dihydro-β-erythroidine required to produce initial relaxation of muscle tension when given intravenously at the rate of 0.5 mg/min was found to vary considerably from animal to animal, ranging from 0.20 to 0.24 mg/kg. With continued administration, the degree of relaxation gradually increases until complete relaxation and abolition of spontaneous movements is attained with a dose of 0.4–0.5 mg/kg. Paralysis of respiratory movements ensues when 0.9–1.2 mg/kg has been injected. In contrast to these findings on cats, intravenous administration of the compound to monkeys (*Macaca mulatta*) at the rate of 1 mg/min was found to produce initial relaxation of muscle tension with 4–6 mg/kg. Practically complete relaxation with loss of spontaneous movements occurs when 7–8 mg/kg has been given, and respiratory movements cease after 8.2–11.4 mg/kg. It appears, therefore, that the amount necessary to produce a given effect varies considerably

[1] Aided by a grant from the National Foundation for Infantile Paralysis.

[2] The erythroidine compounds were supplied by Merck and Co. and the myanesin by E. R. Squibb and Sons.

not only from genus to genus, but also among individuals of any one species.

For the initial studies of the effect of these drugs on increased muscle tension, the standard preparation of the decerebrate cat was utilized. In such an animal it has previously been shown by Smith (*10*) that the administration of either of the two erythroidine compounds causes a gradual and complete relaxation of the rigid extremities without abolition of the tendon reflexes and without paralysis of respiratory movements. The amount required to produce relaxation of muscle tension was found to be approximately the same as in the case of nonanesthetized animals. When the excessive tension is completely abolished, the tendon reflexes are still present and respiratory movements persist. Continued administration of the drug produces paralysis of respiratory movements with a dosage similar to that required in experiments on nonanesthetized animals. With the advent of respiratory paralysis, tendon reflexes are no longer elicitable even though adequate oxygenation is insured by artificial respiration. Retention of the tendon reflexes in the state of abolition of the abnormal muscular tension, and the finding that at this stage the muscle is excited by stimulation of its nerve, suggests the possibility that the initial relaxing effect of the erythroidine compounds is not due to motor end-plate block, but to action of the drug on the central nervous system. Further investigations support this view by revealing that the crossed extensor reflex is abolished before loss of the knee jerk, although in every instance the knee jerk is diminished and exhibits the fatigability seen when the compounds are administered to nonanesthetized animals.

We next directed our attention to myanesin (*o*-toloxy-1,2-propanediol) in order to determine if it possessed a more discrete differential action on neuromuscular mechanisms than had been observed with erythroidine. This compound, when administered in appropriate amounts, has been shown by Berger and Bradley (*1, 2*) and Berger (*3*) to produce muscular relaxation and paralysis in animals anesthetized with chloralose without cessation of respiratory movements and without abolition of the knee jerk. They also found that contraction of the muscle through excitation of its nerve was not abolished except with doses larger than those necessary to produce paralysis of the extremities. Our investigations designed to yield information concerning the locus of action of the drug were first conducted on nonanesthetized cats. Administration of an aqueous solution of myanesin intravenously at the rate of 10 mg/min was found to produce a progressive generalized decrease in muscle tension, resulting in its complete abolition and in the disappearance of all spontaneous movements when approximately 125 mg/kg had been injected. At this stage, respiratory movements continued and the knee jerks were still present, but the flexor withdrawal reflex to noxious cutaneous stimuli was abolished.

In experiments employing decerebrate cats, it was found that myanesin, like erythroidine, produced a progressive relaxation of the muscle tension to the point of complete abolition. The amount required was considerably less than that necessary to abolish tension in the nonanesthetized animal. Even at the stage of complete relaxation, the muscle responded to stimulation of its nerve and knee jerks were elicitable, although both crossed extensor reflex and flexor reflex were abolished.

Analysis of the above experiments naturally led to the conclusion that myanesin had a differential action on central mechanisms, leaving intact the myotatic reflexes as exemplified by the knee jerk, but rendering inactive the reflex path for the flexor and crossed extensor reflexes. Further delimitation of the field of activity was attained by use of spinal animals. In spinal cats, with level of transection through the lower thoracic region, appropriate amounts of myanesin were observed to exert the same discrete differential effect on the spinal reflexes as in the decerebrate animal.

The interpretation of these results in the light of present day concepts of spinal cord structure and function affords information even more definitive concerning the probable locus of action. Current concepts of functional organization of the spinal cord, largely influenced by the investigation of Lloyd (*8, 9*), hold to the view that the crossed extensor reflex, the flexor withdrawal reflex, and other somatic reflexes elicited in response to cutaneous stimuli are mediated over pathways containing internuncial neurons interposed between the entering afferent fibers and the motor neurons innervating the reacting muscles. On the other hand, myotatic reflexes, of which the knee jerks and ankle jerks are well known examples, are considered to be effected by the incoming afferent impulses exciting the somatic motor neurons directly without the intervention of interneurons. Inasmuch as our experiments clearly demonstrate that these several responses which at present are considered to involve internuncial activity are abolished by erythroidine and myanesin in appropriate amount, while the myotatic reflexes remain in effect, it seems likely that both drugs are able to exert a relaxing effect on muscular tension by action on spinal internuncial neurons in such a manner as to produce a partial or complete internuncial blockade. The precise manner by which this is brought about must be determined by further investigations.[3]

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[3] We have recently found that the administration of myanesin to the anesthetized cat abolishes movements of the extremities elicited by electrical stimulation of the motor cortex.

Comments and Communications

Distinction between Acetylcholine-Esterase and Other Choline Ester-splitting Enzymes

During the past few years, choline ester-splitting enzymes have been studied by many investigators. One of the problems involved has been the question of the existence of an esterase specific for acetylcholine—a question of considerable interest in view of the important physiological function of this ester. Much information has been obtained and the facts are, with a few exceptions, in good agreement. However, the picture may appear confusing, due mainly to the lack of a consistent classification and terminology. It is the purpose of this note to propose a classification and a terminology which may contribute to clarifying the situation.

The question of the existence of an esterase specific for acetylcholine was first raised by Stedman, Stedman and Easson (*Biochem J.* 1932, **26**, 2056). These investigators thought that the enzyme which they prepared from horse serum was specific for choline esters, although not for acetylcholine, since it split butyrylcholine at a higher rate than acetylcholine. Later experiments, however, have shown that the esterases present in some sera are not specific for choline esters, although they split them faster than noncholine esters (VAHLQUIST, B. *Skand. Arch. Physiol.*, 1935, **72**, 133; and GLICK D. *J. biol. Chem.*, 1938, **125**, 729; 1939, **130**, 527; 1941, **137**, 357). Alles and Hawes found that the esterase in red blood cells differs markedly from the serum esterase (*J. biol. Chem.*, 1940, **133**, 375). The former is inhibited by high concentrations of acetylcholine in contrast to the latter; it hydrolyzes acetyl-β-methylcholine, which is not split by serum esterase (Glick, *op. cit.*). Richter and Croft (*Biochem J.*, 1942, **36**, 745) confirmed these observations and found the red cell esterase highly specific for acetylcholine. Brain esterase was found to be similar to the red cell esterase (ZELLER, E. A. and BISSEGGER, A. *Helv. chim. Acta*, 1943, **26**, 1619; NACHMANSOHN, D. and ROTHENBERG, M. A. *J. biol. Chem.*, 1945, **158**, 653). These two esterases do not split benzoylcholine (MENDEL, B., MUNDEL, D. B., and RUDNEY, H., *Biochem. J.*, 1943, **37**, 473). Testing a variety of esters and esterases of different tissues, Nachmansohn and Rothenberg showed that the esterase in all conductive tissue, nerve and muscle, and in erythrocytes has a number of well-defined properties. In addition to a well-defined optimum acetylcholine concentration, described previously, they found that the enzyme splits propionylcholine at the same or at a lower rate than acetylcholine, whereas butyrylcholine was shown to be split at a very low rate or not at all. At a concentration optimal for acetylcholine, noncholine esters were split at a very low rate or not at all. The observations included a virtually pure esterase preparation obtained from the electric tissue of *Electrophorus electricus*. In striking contrast to this type of esterase, esterases of other tissues, like that of serum, split propionylcholine at a higher rate than acetylcholine and butyrylcholine at a higher rate than propionylcholine.

The importance of studying the enzyme activity as function of substrate concentration for all esters has been emphasized by K. B. Augustinsson (*Acta. physiol. Scand.*, 1948, **15**, Suppl. 52). Testing the activity as function of substrate concentration, he found that the esterases from conductive tissue and erythrocytes have the same optimum substrate concentration for propionylcholine and butyrylcholine, whereas, e.g., for acetyl-β-methylcholine they have a different optimum (*J. biol. Chem.*, in press). Noncholine esters, like triacetin, show entirely different activity-substrate concentration relationships. At very high substrate concentrations, hydrolysis rate may be even higher than with acetylcholine.

From all these investigations it is obvious that there

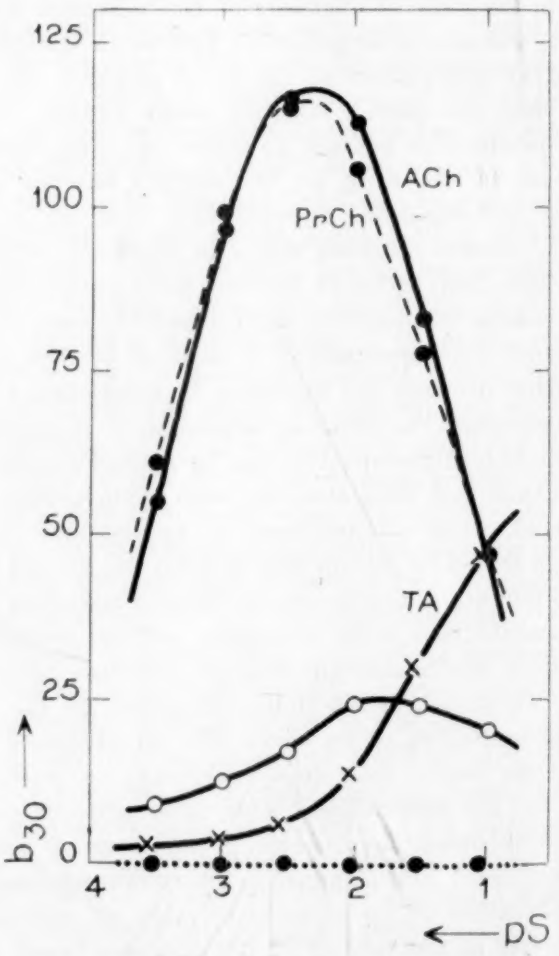

FIG. 1. *Acetylcholine-esterase.* Source: electric tissue of *Electrophorus electricus.* Activity-pS curves for the enzymatic hydrolysis of various esters.

- ●------● Acetylcholine (ACh);
- ●.....● butyrylcholine (BuCh);
- ●----● propionylcholine (PrCh);
- ○——○ DL-acetyl-β-methylcholine;
- ×——× triacetin (TA).

is a type of esterase which is highly specific for acetylcholine, although this specificity is relative and not absolute. Whereas the facts accumulated present a fairly clear picture, no agreement exists concerning the terms

used for the different types of choline ester-splitting enzymes. Zeller distinguishes the enzymes according to the material where they were first found and described. Nachmansohn and Rothenberg referred to the enzyme in conductive tissue and erythrocytes as a cholinesterase specific for acetylcholine, but a term distinguishing this type from other choline ester-splitting enzymes appears desirable. Augustinsson defined as cholinesterase all those esterases which, at substrate concentration optimum

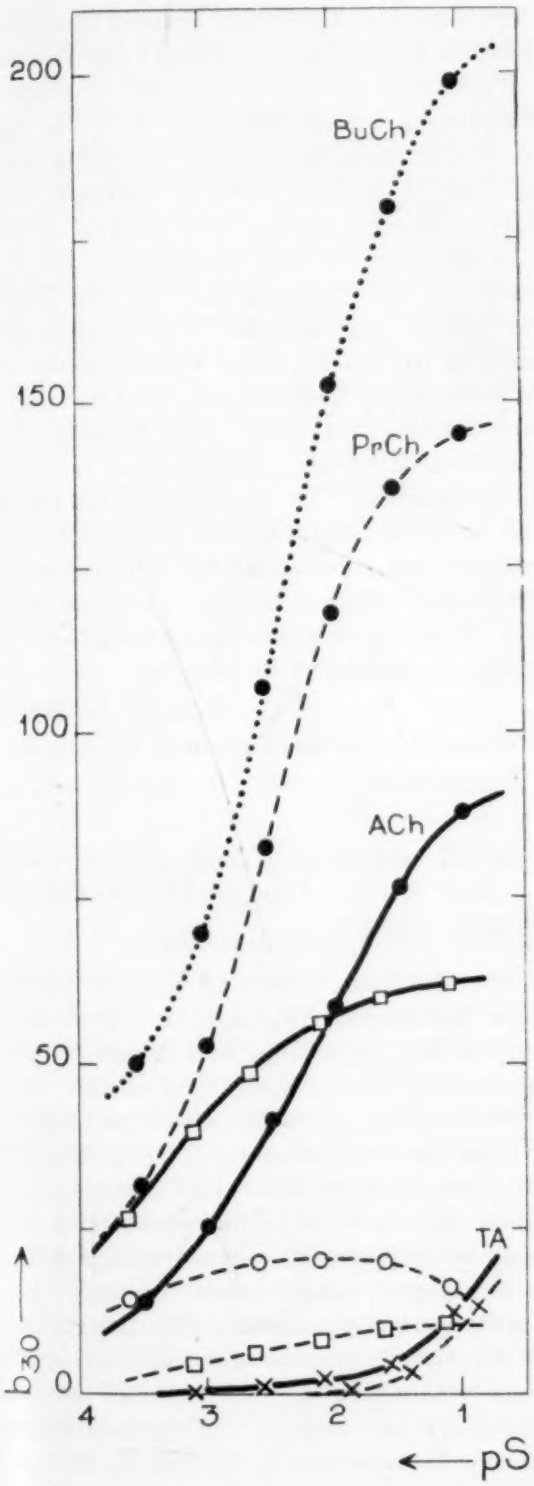

FIG. 2. *Cholinesterase.* Source: human serum. Activity -pS curves for the enzymatic hydrolysis of various esters. Symbols the same as in Fig. 1.

for acetylcholine, split this ester at a higher rate than any other ester. Since he included in this classification the serum esterase, this definition must be revised.

Mendel and his associates introduced the terms "true" and "pseudo" cholinesterase. In contrast to the finding of other investigators, Mendel and Rudney found an optimum rate of acetylcholine hydrolysis by brain esterase to be below 0.0002 M substrate concentration. This optimum rate, at an extremely low acetylcholine concentration, they consider to be the decisive difference of "true" as compared with the "pseudo" cholinesterase, for which they found an optimum of above 0.02 M. These data were based on inadequate techniques (NACHMANSOHN, D. and ROTHENBERG, M. A. *J. biol. Chem.*, 1945, **158**, 653). The experiments were not carried out under optimal conditions, especially in regard to the salt concentrations (AUGUSTINSSON, K. B. *Acta physiol. Scand.*, 1948, **15**, suppl. 52). Also, the expressions "true" and "pseudo" enzyme have met general opposition by enzyme chemists (GLICK, D. *Science*, 1945, **102**, 100).

Since there exists a type of esterase with well-defined properties and highly specific for acetylcholine, we suggest the use of the term acetylcholine-esterase (ACh-esterase) for this enzyme. Its most outstanding characteristics may be summarized as follows: It has a high affinity to acetylcholine, that is, the Michaelis constant is small. The turnover number is high. No other ester is hydrolyzed at a higher rate. The enzyme splits propionylcholine at the same or at a lower rate, butyrylcholine, at a low rate or not at all. Noncholine esters are not split or at a low rate. The enzyme has a low affinity to these latter esters. If the enzyme activity is plotted against log M acetylcholine concentration, a bell-shaped curve is obtained, consistent with the theory of Haldane about inhibition of enzyme by excess of substrate (see Fig. 1). The ACh-esterase occurs in conductive tissue (nerve and muscle) and in erythrocytes. Esterases with similar characteristics occur in a few special cases, like snake venom and *Helix* blood (AUGUSTINSSON, K. B. *Acta physiol. Scand.*, 1948, **15**, suppl. 52), although these enzymes have a few distinct features as compared with the ACh-esterase of conductive tissue.

The properties of acetylcholine-esterase make it possible to distinguish this enzyme from other esterases. There are other esterases, e.g., in some sera, and in the pancreas, which split choline esters at a higher rate than noncholine esters. According to the usual enzymological terminology, these esterases may be called cholinesterases, as was originally proposed by Stedman for the serum esterase. Their physiological substrate is at present unknown and so the name seems at least temporarily appropriate. These esterases as compared with acetylcholine-esterase have a high Michaelis constant for choline esters. They are not inhibited by high concentrations of acetylcholine. Hydrolysis rate increases with increasing length of the acyl chain (from acetyl to *n*-butyryl) (Fig. 2).

It is hoped that the classification and terminology proposed may help to avoid further confusion. The term ACh-esterase, moreover, will associate this type of enzyme with its physiological substrate, at least in nerve and muscle tissue, where a function of acetylcholine appears fairly well established.

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Book Reviews

Contributions to the anthropology of the Soviet Union. (Publ. 3947.) Henry Field. (Compiler.) Washington, D. C.: Smithsonian Institution, 1948. Pp. vii + 244. (Illustrated.) $2.00.

This volume, which is a compilation of recent Soviet data pertaining to the archaeology and physical anthropology of the USSR, is certainly the most comprehensive and ambitious of Dr. Field's long and very impressive list of valuable publications dealing with the study of man in the lands administered by the Russians. Dr. Field acquired the bulk of his material in Moscow and Leningrad during June and July, 1945, when he was a delegate to the Jubilee Session celebrating the 220th anniversary of the founding of the Academy of Sciences of the USSR by Peter the Great. These meetings, known as the All-Union Conference on Archaeology, were attended by a total of 156 delegates. In chapter I, Dr. Field has ably summarized the scope and significance of the most important papers presented at this conference. Collectively, these reveal that Soviet scientists are now prepared to admit without reservation the fact that archaeological investigations in the USSR during the past twenty-five years have contributed a wealth of new and important material bearing on the problem of the origin of the Slavs and the development of humanity from Palaeolithic times to the Middle Ages.

V. V. Bunak, who prepared chapter II, has included brief accounts of the fossil human remains found at Teshik-Tash, Southern Bukhara, and Kiik-Koba, Crimea, as well as some general comments regarding current research on skeletal material from various sections of the USSR, racial studies in Siberia and the Caucasus, and investigations of a joint anthropological-medical nature on growth changes in the human body. Chapters III and IV are devoted entirely to archaeology, the former being a translation of a paper by P. P. Efimenko and N. A. Beregovaia published in 1941 on "Palaeolithic Sites in the USSR." It presents a comprehensive summary of some 304 Palaeolithic localities in European Russia and Siberia based on the data available during 1938; however, all bibliographic references have been deleted from the translation. Under the heading "Miscellanea Archaeologia," the title of chapter IV, Dr. Field has assembled a series of items of archaeological interest from the Ukraine, Crimea, Black Sea coast, Caucasus, Armenia, Don region, Volga region, Urals, Russian Turkestan, and Siberia. In many instances these supplement notes were previously published by the author on the same general subject.

More than one-half of the book (total: 131 pages) consists of a series of studies based on the results obtained by Soviet physical anthropologists, together with observations recorded by Dr. Field himself in the Soviet Union during September and October, 1934. The most important of these studies, which are grouped under the general heading "Miscellanea Anthropologia" (chapter V), concern the peoples of the Western Pamirs, the Mountain Tajiks, the peoples of Uzbekistan, the Kazakhs, the Turkomans, and the Turkish peoples of the USSR. Other sections of this chapter are devoted to a description of the so-called Tardenoisian skeleton from Fatma Koba, Crimea, a summary of the palaeo-anthropology of the Lower Volga area (third millenium B. C. to the third century A. D.), the results of various craniological studies, including 14th and 15th century Tartars, Kalmyks, Ulchi, Yakuts, Orochis, and Aleuts, as well as short discussions on the origin of the Mongols and Eskimos, and blood groups in the Caucasus and of the Turkomans. The extraordinary range and wealth of material presented here testifies to Dr. Field's broad understanding of racial problems, particularly those bearing on Asia. All American anthropologists will await with interest the forthcoming publication of his *Contributions to the anthropology of the Caucasus.* In the meantme, the Smithsonian Institution is to be congratulated for bringing out the present volume, and commended for the excellence of the format employed. This is a book which no student of either Old World prehistoric archaeology or physical anthropology can afford to be without.

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Readings in the history of psychology. Wayne Dennis. (Ed.) New York: Appleton-Century-Crofts, 1948. Pp. xi + 587. $4.75.

This is the first such volume of selected readings, all presented in the English language, to appear since 1912. It is timely in that psychology has changed much in the last forty years. The compiler has chosen 61 classics that are based mostly on investigation and experimentation. Articles are reprinted in full; monographs and books are represented by excerpts. The arrangement is chronological by date of publication, and full bibliographical data are given. Some readings are as short as one or two pages; none exceeds eighteen.

There are two readings from Aristotle; all the others date from the 17th century or later; half are more recent than Wundt's founding of experimental psychology in 1879. Thirty are 19th century and eighteen 20th century contributions, the latest by C. L. Hull in 1930. Two to four selections are made from the works of some of the outstanding figures in the history of psychology—for example, Thomas Young, E. H. Weber, Helmholtz, Galton, Cattell, Thorndike, Köhler, Lashley, and Binet and Simon. Such emphasis seems appropriate in assembling scientific material from a total of 46 contributors to the more immediate background of modern psychology as usually taught in American colleges and universities.

In arriving at his final selection, the compiler, on the basis of his own teaching experience, first drew up a list of 48 titles, which he submitted to a group of 14 psychologists for criticism and suggestions on serious omissions. The final list of 61 choices represents his attempt at a balance between the wealth of material and the space available in one volume. He frankly admits that " . . . many important contributions to psychology could not be included." Although some who teach psychology will doubtless discover regrettable omissions, they will, on the other hand, probably find here much if not most of what they would wish to have conveniently available to their elementary students. Historical classics in scientific literature become increasingly rare and original texts suffer from exposure to the wear and tear of reading by large classes. It is important that worthy classics should be reprinted in accessible form and the compiler and publisher have made a real contribution toward this end. The volume has a good index for names, but unfortunately has none for psychological topics. It has been planned more for supplementary use in course teaching than for individual study in working up the background of special fields and subjects in psychology. A bibliography of texts and articles on the history of psychology might appropriately have been included; however, general students of this modern scientific subject will benefit from the well-arranged compilation.

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Aufgabe der Psychologie: Eine Geschichte ihrer Probleme. (The Task of Psychology: A History of its Problems.) Paul v. Schiller. Vienna, Austria: Springer-Verlag, 1948. Pp. 233. $4.00.

This book is an enlarged and revised translation of a Hungarian original published in 1940 by the Hungarian Academy of Sciences. It endeavors to reestablish the unity of psychology by pointing out its historical centering about the concept of action (*Handlung*), with its prime emphasis on overt behavior and adjustment to environment rather than on the introspective aspect of human responses. The latter aspect is seen as subordinate to the former, somewhat in the manner of the American functionalists and of Bühler. Ancient and medieval writers, such as Plato, Aristotle, Augustine, and Thomas receive more than their usual share of attention, and with them the philosophical issues tied in with the history of psychology. The originality of the author is at its best in these chapters.

Much of the presentation of modern American writers, primarily such "molar behaviorists" as Tolman and Lashley, is obviously meant to be informative on a relatively elementary level and adapted to the needs of the author's Hungarian students—whose only practicable access to the type of foreign literature involved may well have been this book. After a discussion of psychoanalysis, behaviorism, *Gestalt* psychology and some of the older psychological systems, the author concludes with a general discussion of the role of psychological needs and motivation in the framework of action.

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Storia naturale del sesso. Emanuele Padoa. Turin, Italy: Giulio Einaudi, 1948. Pp. 560. (Illustrated.) 2100 lire.

This book on the biology of sex, written during the war under very difficult conditions, is one of the most remarkable biological works that have come out in Italy during these years. Dr. Padoa, professor of biology at the University of Siena, has worked for many years in these problems and has contributed some fundamental researches to the problem of sex biology. He has done an excellent job in presenting here in a clear and simple way problems that are sometimes made very complex. One of the book's most outstanding features is its wealth of information; the reader finds here all the most interesting data on sex biology in plants and animals, always interpreted by the use of modern genetics. Problems of genetics of sex, cytogenetics, physiology of reproduction, phenotypic and genotypic sex determination, chemistry of sex hormones in vertebrates, experimental sex reversal, and sex evolution are all discussed.

To examine the subjects discussed in the chapter on genetic determination of sex, for example, there are sections on genetic determination of diplonts, crossing of a heterozygote with a recessive homozygote, numerical ratio of sex, sex in identical twins, sex chromosomes, inheritance of characters linked to sex (man, *Drosophila*, cat, *Melandrium*, chicken, butterflies), equilibrium of sex genes, genetic distribution in the gametophytes of the aplodiplonts, determination of sex with zygotic reduction, sex in hermaphrodites, and sterility in plants and animals. All the other nine chapters of the book provide just as thorough treatment of their subjects.

Another interesting feature of the book is its reporting of research on sex biology done in Europe during the war years and not yet well known in the United States—for example, the works of Hartmann and his school in Germany, on the physiology of reproduction in the echinoderms, and findings in the same field at the Zoological Station of Naples during the war years.

In spite of the vast amount of material, the real problem is never lost in details, but always emerges clear and vivid.

At the end of each chapter there are references to review articles and other collateral material, besides the references to original literature for those who wish to go more deeply into a particular subject.

This book may be well recommended both to biologists who want to keep informed of new problems and to students who want to become acquainted with this part of modern biology.

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Scientific Book Register

BURK, R. E., and GRUMMITT, OLIVER. (Eds.) *High molecular weight organic compounds.* (Frontiers in Chemistry, Vol. vi.) New York: Interscience, 1949. Pp. ix + 330. (Illustrated.) $5.50.

COBURN, ALVIN F., and YOUNG, DONALD C. *The epidemiology of hemolytic streptococcus during World War II in the United States Navy.* Baltimore: Williams & Wilkins, 1949. Pp. 229. (Illustrated.) $4.00.

CONN, HOWARD F., *et al.*, (Ed.) *Current therapy 1949: latest approved methods of treatment for the practicing physician.* Philadelphia: W. B. Saunders, 1949. Pp. xxxii + 672. $10.00.

COURNAND, ANDRÉ, BALDWIN, JANET S., and HIMMELSTEIN, AARON. *Cardiac catheterization in congenital heart disease: a clinical and physiological study in infants and children.* New York (22): Commonwealth Fund, 1949. Pp. viii + 108. (Illustrated.) $4.00.

COXETER, H. S. M. *The real projective plane.* New York: McGraw-Hill, 1949. Pp. x + 196. (Illustrated.) $3.00.

DAVID, F. N. *Probability theory for statistical methods.* New York-London: Cambridge Univ. Press, 1949. Pp. ix + 230. $3.50.

FINE, JACOB. *Care of the surgical patient: including pathologic physiology and principles of diagnosis and treatment.* Philadelphia: W. B. Saunders, 1949. Pp. xvi + 544. (Illustrated.) $8.00.

GATES, FRANK C. *Field manual of plant ecology.* New York: McGraw-Hill, 1949. Pp. xv + 137. (Illustrated.) $3.00.

GERARD, R. W. *Unresting cells.* (Reissue.) New York: Harper, 1949. Pp. xv + 439. (Illustrated.) $4.00.

GOETZEL, CLAUS G. *Treatise on powder metallurgy: technology of metal powders and their products.* (Vol. 1.) New York: Interscience, 1949. Pp. xxvii + 778. (Illustrated.) $15.00.

GRIFFIN, CARROLL WARDLAW. *Inorganic quantitative analysis.* Philadelphia-Toronto: Blakiston, 1949. Pp. xiv + 368. (Illustrated.) $4.50.

HABELL, K. J., and COX, ARTHUR. *Engineering optics: the principles of optical methods in engineering measurement.* New York (19): Pitman Publ., 1948. Pp. xiv + 411. (Illustrated.) $7.50.

HALL, MARGUERITE F. *Public health statistics.* (2nd ed., rev.) New York: Paul Hoeber, 1949. Pp. xxvii + 441. (Illustrated.) $7.50.

HARDENBURG, E. V. *Potato production.* Ithaca, New York: Comstock, 1949. Pp. xi + 270. (Illustrated.) $3.00.

HARTMAN, FRANK A., and BROWNELL, KATHARINE A. *The adrenal gland.* Philadelphia: Lea & Febiger, 1949. Pp. 581. (Illustrated.) $12.00.

HATCH, MELVILLE H. *A century of entomology in the Pacific Northwest.* Seattle: Univ. of Washington Press, 1949. Pp. v + 42. (Illustrated.) $1.50.

HATCHER, WILLIAM H. *An introduction to chemical science.* (2nd ed.) New York: John Wiley, 1949. Pp. x + 449. (Illustrated.) $4.00.

HUNTER, GEORGE W., and HUNTER, F. R. *College zoology.* Philadelphia: W. B. Saunders, 1949. Pp. xi + 821. (Illustrated.) $5.50.

KAUFMAN, WILLIAM. *The common form of joint dysfunction: its incidence and treatment.* Brattleboro, Vt.: E. L. Hildreth, 1949. Pp. xi + 208.

KING, G. BROOKS, and CALDWELL, WILLIAM E. *The fundamentals of college chemistry.* New York: American Book, 1949. Pp. viii + 536. (Illustrated.) $4.00.

KRANTZ, JOHN C., JR., and CARR, C. JELLEFF. *Pharmacologic principles of medical practice.* Baltimore: Williams and Wilkins, 1949. Pp. xv + 980. (Illustrated.) $10.00.

NICOLAI, GEORG FR. *La seguridad cientifica: cuatro discursos del metodo de las ciencias naturales.* Santiago: University of Chile Press, 1948. Pp. 245.

RANGAPPA, K. S., and ACHAYA, K. T. *The chemistry and manufacture of Indian dairy products.* Bangalore, India: Bangalore Prntng. & Pblg., 1948. Pp. vi + 189. Price Rs. 10/–.

RIDER, PAUL R. *Intermediate algebra for colleges.* New York: Macmillan, 1949. Pp. x + 242. $2.75.

ROSEBURY, THEODORE. *Peace or pestilence: biological warfare and how to avoid it.* New York: Whittlesey House (McGraw-Hill), 1949. Pp. 218. $2.75.

SOMMERFELD, ARNOLD. (Trans. by Ernest G. Straus.) *Partial differential equations in physics.* New York: Academic Press, 1949. Pp. xi + 335. $5.80.

SYNGE, JOHN L., and GRIFFITH, B. A. *Principles of mechanics.* (2nd ed.) New York: McGraw-Hill, 1949. Pp. xvi + 530. $5.00.

WEISER, HARRY BOYER. *A textbook of colloid chemistry.* (2nd ed.) New York: John Wiley; London: Chapman & Hall, 1949. Pp. x + 444. (Illustrated.) $5.50.

WILSON, A. J. C. *X-ray optics: the diffraction of X-rays by finite and imperfect crystals.* London, W.C.2: Methuen, 1949. Pp. vii + 127. (Illustrated.) 6/.

ZWORYKIN, V. K., and RAMBERG, E. G. *Photoelectricity and its application.* New York: John Wiley; London: Chapman & Hall, 1949. Pp. xii + 494. (Illustrated.) $7.50.

———. *Child health services and pediatric education.* (Report of the Committee for the Study of Child Health Services, American Academy of Pediatrics, with the cooperation of the U. S. Public Health Service and the U. S. Children's Bureau.) New York: Commonwealth Fund, 1949. Pp. xxv + 270. (Illustrated.) $3.50.

NEWS and Notes

An amendment to the independent offices appropriations bill HR 4177, calling for full FBI investigations of all fellows under the program of the Atomic Energy Commission, has been prepared by Senator O'Mahoney. Scheduled for introduction when the bill comes before the Senate, it reads as follows: "No part of any appropriation contained in this title for the Atomic Energy Commission shall be used to confer a fellowship on any person who advocates or who is a member of an organization or party that advocates the overthrow of the Government of the United States by force or violence or with respect to whom the Attorney General finds, upon investigation and report by the Federal Bureau of Investigation on the character, associations, and loyalty of whom, that reasonable grounds exist for belief that such person is disloyal to the Government of the United States: *Provided,* That any person who advocates or who is a member of an organization or party that advocates the overthrow of the Government of the United States by force or violence and accepts employment the salary, wages, stipend, or expenses for which are paid from any appropriation contained in this title shall be guilty of a felony and, upon conviction, shall be fined not more than $1,000 or imprisoned for not more than one year, or both: *Provided further,* That the above penal clause shall be in addition to, and not in substitution for, any other provisions of existing law."

Another amendment to the Atomic Energy Act of 1946 was introduced by Senator McMahon as S. 2215 on July 7, placing further limitations upon the Commission. This bill proposes that "the authorization of Congress shall be required . . . with respect to the total amount of money and contract authorization annually requested . . . before the appropriation of any funds for the Commission's use shall be authorized." By means of this amendment Congress would, in effect, acquire direct control over every "substantial activity, project, or operation undertaken or to be undertaken in accordand with the provisions of this Act."

The National Science Foundation bill, H. R. 4648, which has been approved by the Committee on Interstate and Foreign Commerce, is being held up in the Rules Committee. Apparently that committee is reluctant to bring the bill to the House of Representatives for vote because of the current economy drive. Members of the Rules Committee are: *Democrats*—Adolph J. Sabath (Illinois) chairman, E. E. Cox (Georgia), Howard W. Smith (Virginia), William M. Colmer (Mississippi), Ray J. Madden (Indiana), John E. Lyle, Jr. (Texas), John McSweeney (Ohio), and James J. Delaney (New York); *Republicans*—Leo E. Allen (Illinois), Clarence J. Brown (Ohio), James W. Wadsworth (New York), and Christian A. Herter (Massachusetts).

Detlev W. Bronk, chairman of the National Research Council and president of Johns Hopkins University, delivered the annual Croonian Lecture of the Royal Society of London on June 30. Dr. Bronk spoke on the rhythmic action and respiration of nerve cells.

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Carroll L. Birch, associate professor of blood diseases and tropical medicine at the University of Illinois Medical School, has left on a six-month expedition to the west coast of Africa. During her stay in Africa, Dr. Birch will make side trips to the Belgian Congo, the Gold Coast, Liberia, and Nigeria, and into remote sections of French Equatorial Africa to obtain specimens of disease-spreading organisms for use in courses at the medical school. She will also do research work on natives with various forms of malaria and sleeping sickness.

Frank G. Young, chairman of the Department of Chemistry, University College, London, has been appointed professor of chemistry at Cambridge University. Dr. Young recently visited the U. S. to participate in a symposium on diabetes at the Army Medical Department Research and Graduate School.

D. J. Hughes, division director of the Experimental Nuclear Physics Research Division, Argonne National Laboratory, has resigned to accept a position as senior physicist at the Brookhaven National Laboratory, Upton, New York.

J. K. F. Holtfreter, of the Zoology Department at the University of Rochester, will be guest lecturer for the summer session at the Department of Histology and Embryology of the Faculty of Medicine, University of Montevideo, Uruguay.

Visitors to U. S.

Recent visitors at the National Bureau of Standards were **H. Powell,** research chemist, Anglo Iranian Oil Company, Research Station, Sunbury-on-Thames, England; **James Brown,** lecturer in mechanical engineering and applied mechanics at the Royal Technical College of Glasgow, Scotland; **Hernando Posada,** general manager, Instituto de Crédito Territorial, Bogota, Colombia; **Gerardo A. Imperial,** professor of chemistry at Silliman University, Philippine Islands; **Gordon Weston,** assistant director of the British Standards Institution, London; **W. W. Vardssieau,** chemist, National Research Council of Holland; **Virginia E. Marting,** research assistant, Cavendish Laboratory, Cambridge University, England.

The National Vitamin Foundation has announced six new grants-in-aid for research in problems of nutrition in health and disease. The awards, effective July 1, were made to *Leonard J. Goldwater* and *Maurice E. Shils,* Columbia University School of Public Health; *Winslow T. Tompkins,* Pennsylvania Hospital, Philadelphia; *Ernest Geiger,* University of Southern California School of Medicine; *Robert W. Heinle* and *Thomas P. Singer* (separate grants), both of Western Reserve University

School of Medicine; and *Willard A. Krehl,* Yale University.

The Carnegie Corporation has made a three-year grant of $90,300 to the University of Minnesota for research in the social sciences, involving the fields of political science, psychology, and anthropology. The program will be carried on through the university's Laboratory for Research in Social Relations. Further information may be secured from John G. Darley, Assistant Dean of the Graduate School, University of Minnesota, Minneapolis 14.

The Honor Scroll Award of the American Institute of Chemists will be presented to Otto Eisenschiml, president of Scientific Oil Compounding Company, at a meeting of the Chicago Chapter on October 7. Dr. Eisenschiml is being honored for his work in the chemistry of vegetable oils.

The National Research Council of Canada has awarded fellowships totaling $75,500 to 38 graduates in medicine to enable them to pursue postgraduate research during 1949–50. The fellowships vary in amounts from $1,500 to $2,500. The grantees represent 11 universities, including one in Australia, one in Scotland, and one in China. Eight of the research centers are located in Canada, five in the United States, and one in England.

Colleges and Universities

A group of Princeton University physicists will spend six weeks aboard the *U.S.S. Norton Sound* making cosmic ray experiments at sea. The experiments will be conducted by two scientifically independent groups under the direction of John R. Winckler and George Neilson Whyte. The Winckler group will use balloons for a determination of the energy distribution of the primary cosmic rays and relative number of rays having positive and negative electrical charges. The Whyte group will undertake two experiments: number and size of nuclear explosions due to cosmic ray particles as a function of altitude, and determination of the number of slow neutrons present in the atmosphere as a function of altitude. It is expected that information obtained will contribute toward determining the source of cosmic rays and give basic information necessary for the study of very high energy nuclear interactions and the forces which hold the nucleus together.

The New York University College of Medicine has received a shipment of snails infected by a parasite which transmits the tropical disease schistosomiasis. Claude H. Barlow, parasitologist, brought the snails from Egypt, where they infest the irrigation canals, serving as host for the larva worm. Dr. Barlow deliberately infected himself with the disease several years ago in order to study it and received the Medal for Merit last year in recognition of his contribution to science. Fifty-four percent of all Egyptians are affected by the disease. The university will conduct an intensive research project to find methods of eradicating schistosomiasis.

Meetings and Elections

The United Nations announces that arrangements for reserved seating space at its **Scientific Conference on the Conservation and Utilization of Resources** (see *Science* April 29, page 452) will be made for those who request such reservations in advance of the conference, August 17–September 6. The meetings will be open to the public; and attendance of scientists and other technical experts interested in resource use and development is particularly welcome. Those interested should write directly to the United Nations, Lake Success, New York, addressing letters to the attention of Alfred J. Van Tassel, Executive Secretary, UNSCCUR. If seating space is available, it will be possible to make reservations by telephone one day in advance, up to the last day of the conference. Calls should be made to D. J. Walsh, UN Admissions Officer, Fieldstone 7-1100, Extension 2126 or 2156. Not more than two reservations can be made by a telephone call.

The Meteoritical Society will hold its twelfth annual meeting September 6–7, at the University of Southern California, Los Angeles. Chairman of the program committee is John A. Russell, Department of Astronomy, University of Southern California, Los Angeles 7.

The fourth meeting of the **Conference on Low Temperature Calorimetry** will be held at the Massachusetts Institute of Technology on September 10 in conjunction with the **International Conference on the Physics of Very Low Temperatures,** scheduled September 6–10. Further details can be supplied by J. C. Slater, Department of Physics, M. I. T., Cambridge, Massachusetts.

The summer meeting of the **Northeastern Division, American Phytopathological Society** will consist of a tour through various parts of New Jersey September 8–9. Diseases of various crops will be studied in the field, particularly those crops unique to New Jersey or occurring on a large scale in that state.

The Ninth International Congress of Psychotechnology will meet in Berne, Switzerland, on September 12–17, with Henri Piéron as president. Psychologists wishing to send exhibits of charts, tests, or recent books for display should correspond with Harold Seashore, 522 Fifth Avenue, New York 18. For information regarding the congress and for registration, write to Mme. Dr. Franziska Baumgarten, Thunstrasse 35, Berne, Switzerland.

The Illuminating Engineering Society will hold its national technical conference September 19–23 at French Lick, Indiana. A review of lighting achievements of the past year will comprise the program.

A conference on elementary particles will be held in the Department of Natural Philosophy, University of Edinburgh, November 14–16. Further information will be obtainable about the end of August from A. Nisbet, Department of Mathematical Physics, University of Edinburgh, Drummond Street, Edinburgh 8.

The Union Americana de Medicina del Trabajo (American Union of Occupational Medicine), composed

of specialized medical societies of 17 American republics, will hold its first congress in Buenos Aires in December. José Pedro Reggi, professor of the Faculty of Medical Science of Buenos Aires, is director general of the union. Its official publication, *Medicina del Deporte y del Trabajo*, is printed in Spanish, English, and Portuguese. Further information regarding the congress may be obtained from the General Secretary, Arenales 981, T. E. 42, Callao 1015, Buenos Aires.

At the 15th annual meeting of the **American Malacological Union** held at the University of Miami, Florida, June 16–18, the following officers were elected: president, Fritz Haas, Chicago Museum of Natural History; vice president, Joseph P. E. Morrison, Smithsonian Institution; second vice president, John Q. Burch, Los Angeles; treasurer, Harold R. Robertson, Buffalo; secretary, Mrs. Harold R. Robertson, Buffalo Museum of Science.

The Pacific Division of the American Malacological Union held its second annual meeting in Long Beach, California, June 10–12. Officers elected were chairman, John Q. Burch, Los Angeles; vice chairman, Leo G. Hertlein, California Academy of Sciences; secretary-treasurer, Wendell O. Gregg, Los Angeles.

The American Institute of Electrical Engineers elected the following new officers at its annual meeting held June 22: president, James F. Fairman, vice president of the Consolidated Edison Company of New York, Inc.; vice presidents, C. G. Veinott, Lima, Ohio; W. J. Peeley, Durham, North Carolina; W. M. DuVall, Boulder, Colorado; Ralph A. Hopkins, Los Angeles, California; A. H. Frampton, St. Catherines, Ontario, Canada. W. I. Slichter, of New York City, was reelected treasurer.

The Oak Ridge Institute of Nuclear Studies named the following officers at its board of directors' meeting on June 28: president, Paul M. Gross, vice president, dean of the graduate school, and professor of chemistry at Duke University; vice president, J. W. Beams, director of the School of Physics at the University of Virginia; chairman of the 24-man council of the institute, J. Harris Purks, Jr., professor of physics at Emory University on leave as director of the University Center in Georgia; vice chairman of the council, L. A. Pardue, dean of the Graduate School of the University of Kentucky. All of the officials elected retain their university positions and duties.

The council also elected five new universities to membership in the institute. They are Mississippi State College, North Carolina State College, Rice Institute, Virginia Polytechnic Institute, and the University of Oklahoma.

The American Psychiatric Association named the following officers at its 105th annual meeting in Montreal: George S. Stevenson, medical director of the National Committee for Mental Hygiene, New York City, president; John C. Whitehorn, professor of psychiatry, Johns Hopkins University, president-elect; Leo H. Bartemeir, Detroit, secretary; and Howard W. Potter, William Alanson White Clinic, Brooklyn, New York, treasurer.

The Alabama Academy of Science has elected the following officers for the ensuing year: president, C. A. Basore, Alabama Polytechnic Institute, Auburn; president-elect, H. E. Wilcox, Birmingham Southern College; secretary-treasurer, William T. Wilks, Troy State Teachers College, Troy; and councilor of the AAAS, Katherine Vickery, Alabama College, Montevallo.

The American Society of Plant Physiologists has elected the following officers for the year beginning July 1: president, James Bonner, California Institute of Technology; vice president, C. H. Wadleigh, U. S. Regional Salinity Laboratory; and secretary, T. C. Broyer, University of California at Berkeley.

NRC News

The National Research Council has announced the award of 19 **fellowships in the medical sciences.** These fellowships are supported by an appropriation of the Rockefeller Foundation and are designed to give special facilities for training and experience in research to recent graduates who are not yet professionally established, but who wish to devote themselves to an investigative career in the medical sciences. The 12 new fellows for the year 1949–50, the institutions at which they will receive their training, and the subjects of their research are: *Henry T. Bahnson,* University of Rochester, cardiorespiratory physiology; *Harold W. Baker,* Vanderbilt University, bile acid concentration in serum and ability of the liver to conjugate bile salt administered in the unconjugated form in health and in liver disease; the effect of cystine, methionine, and choline on damaged livers; *Richard J. Cross,* Public Health Research Institute of the City of New York, intermediary metabolism in laboratory animals; *Marion E. Lahey,* University of Utah, the pathogenesis of the anemia of infections; *Alexander Leaf,* Massachusetts General Hospital, the renal control of sodium excretion in the edematous subject with cardiac or kidney disease; *Lionel Manson,* Western Reserve University, biosynthesis and breakdown of pentose and desoxypentose nucleosides in microorganisms; *John B. Neilands,* Karolinska Institute, Stockholm, Sweden, enzymes and intermediary metabolism; *Arnold S. Relman,* Evans Memorial Hospital, Boston, relation of renal hemodynamics to electrolyte excretion; *Alexander Rich,* California Institute of Technology, characterization of proteins by physical methods; *Martin P. Schulman,* University of Pennsylvania, isolation of the individual enzymes and cofactors concerned with the synthesis of tissue proteins; *W. Eugene Stern,* Johns Hopkins Hospital, neurophysiological studies in cerebral circulation in primates; and *John M. Weller,* Harvard Medical School, factors affecting the rate of exchange of potassium between the intracellular phases and the metabolic processes which maintain the potassium gradient. The board has renewed seven fellowships for a second year.

The Rockefeller Foundation has also provided the Research Council with funds in support of the **Welch Fellowships in Internal Medicine.** These fellowships are of senior grade, and are intended to provide a prolonged period of advanced training for individuals of proven ability in research. Two new fellows were appointed: *Irving M. London,* for the term 1949–52 at Columbia University, for research in forphyrin metabolism; and *Charles R. Park,* for the term 1949–52 at Washington University, for research in hormonal influences on the hexokinase reaction in muscle. Two other fellowships were renewed.

The National Foundation for Infantile Paralysis, Inc., has made available a grant in support of **research fellowships in fields related to infantile paralysis.** Under this appropriation, fellowships are offered to provide opportunities for training and research in those basic medical sciences which will be of particular value in furthering progress in the field of orthopedic surgery; a second group of fellowships is available for special experience in the study of virus diseases. The following new fellows were appointed for training during 1949–50 at the institutions and in the fields named: *Thomas D. Brower,* University of Chicago Clinics, effects of age and disease on the morphological and biochemical changes in cartilage; *Thomas T. Crocker,* Yale University, purification and study of elementary bodies of the virus of meningopneumonitis; and *Eugene R. Mindell,* University of Chicago Clinics, chondrodystrophies. **Senior fellowships in the fields of clinical neurology, epidemiology, orthopedic surgery, pediatrics, and virus diseases** are also offered under this grant. These senior fellowships are open to men and women who have already shown definite achievement in research. Those appointed in 1949, their terms, training institutions, and subjects are: *Edward H. Ahrens, Jr.,* 1949–50, Rockefeller Institute for Medical Research, New York City, chemical studies of the serum phospholipids and the protein-bound fatty acids; *Calderon Howe,* 1949–51, Harvard Medical School, physical-chemical characterization of the red cell protein, with particular reference to various antigenic or receptor components; *Edward H. Kass,* 1949–52, Thorndike Memorial Laboratory, Boston City Hospital, (1) the cultivation of strains of poliomyelitis virus on tissue culture media, (2) isolation and purification of selected viruses of different immunologic behavior, and (3) disturbance in electrolyte patterns in patients with proven severe viral infection; *Paul S. Rubin,* 1949–50, Johns Hopkins Hospital, mechanisms of calcification; *Charles W. Watson,* 1949–52, Boston City Hospital and Harvard Medical School, relationship between frontal cortex and the thalamus of man; and *Clark D. West,* 1949–50, University of Cincinnati, (1) calculation of proximal and distal tubular osmotic work of the normal kidney during osmotic diuresis, (2) renal insufficiency during dehydration in experimental animals, and (3) osmotic limitations of the nephritic kidney. Five other fellows will work during the coming year under appointments granted in previous years or under renewed appointments made in 1949.

CARE's program to replenish the war-damaged libraries of Europe and the Far East with English-language books (see *Science,* May 6, page 476) will begin this month. A preliminary list of 1,200 titles has been selected by a committee, headed by Luther Evans, Librarian of Congress. The present categories include health and welfare, medicine, dentistry, applied science, nursing, agricultural science, veterinary science, and English language instruction. Additional categories may be included later. The books will be delivered abroad at the publishers' list price with no extra charge for transportation or distribution. Only new books will be sent.

In addition to Dr. Evans, the committee members are John M. Cory, executive secretary, American Library Association; Alice Dulany Ball, executive director, United States Book Exchange; John F. Fulton, Sterling professor, Yale Medical School; and R. R. Hawkins, chief of the Science and Technology Division, New York Public Library.

Contributions may be sent to CARE Book Program, CARE, 20 Broad Street, New York 5. Donors of $10 or more may specify country, type of institution, and category of books, although CARE hopes that the majority of contributions will be undesignated.

A new bimonthly periodical, *Iodine Abstracts and Reviews,* is being prepared by the Iodine Fellowship at Mellon Institute, Pittsburgh, to provide summaries of scientific and technical literature relating to the uses of iodine and its compounds in chemistry and in the industries. From time to time there will be presented reviews of specific phases of the science and technology of iodine. Complimentary subscriptions may be secured by addressing the Iodine Educational Bureau, Inc., 120 Broadway, New York 5.

Make Plans for—

United Nations Scientific Conference on the Conservation and Utilization of Resources, three-week session beginning August 17, Lake Success, New York.

Pennsylvania Academy of Science, August 19–20, Stroudsburg and Buck Hill Falls, Pennsylvania.

1949 Plant Science Seminar, August 21–27, University of Wisconsin School of Pharmacy, Madison.

Symposium on Modern Physics, August 22–September 2, Oak Ridge, Tennessee.

American Institute of Electrical Engineers, Pacific general meeting, August 23–26, Fairmont Hotel, San Francisco, California.

2nd International Biometric Conference, August 30–September 2, University of Geneva, Switzerland.

International Union of Chemistry, 15th conference, September 6–10, Amsterdam, Holland.